THE LANCET.

VOL. I.] LONDON, SATURDAY, DECEMBER 22, 1838. [1838-39.

COURSE OF

LECTURES ON THE BLOOD,

AND ON

THE CHANGES WHICH IT UNDERGOES DURING DISEASE

Delivered at the College of France in 1837-8,

BY

M. MAGENDIE.

LECTURE XII.

Reflux movement of the blood in its vessels.—Movement of the blood in the capillaries.—Peculiarities of the capillary circulation.—Appearances and properties of fibrine.—Formation of nebulous and secondary clots.—Vascularity of the clot.—Physiology of the coagulation of the fibrine.

GENTLEMEN:—I shall conclude the subject of the movement of the blood in the arteries and veins by some considerations on the progression of the liquid in the opposite of the normal direction. The same tube that conveys it from the centre to the circumference, may, under certain circumstances, convey it from the circumference to the centre. Two mechanical agencies appear to concur principally in the production of this reflux of the liquid,—the elastic retraction of the walls of the vessels at the moment the left ventricle expands, and anastomotic communications. When an artery is cut across, the upper end retracts on itself, as well as that which corresponds to the heart, and consequently the blood rushes towards the points where there is least resistance. It will, therefore, chiefly direct itself towards the open orifice. But the flow of blood will not stop the moment the coats of the vessel retract; the anastomotic branches opening into its interior keep a constant current in movement within it. The blood travels freely in the contrary direction to that of its natural course towards the solution of continuity, more especially where several trunks open into each other, as at the base of the skull.

Surgeons who have recommended the ligature of both ends of a divided artery, have given us a wise precept; they have not described the physiological foundation for it, however. I propose measuring the force with which the blood tends to obey the retrograde force, with the instrument we have already so frequently employed. The experiment is quite new; I will have the hæmodynamometer applied to the primitive carotid, a ligature having been previously placed round it; in consequence of the anastomotic communications which unite this vessel with the opposite carotid and both vertebral arteries, it is probable that we shall find about the same amount of pressure in its superior and inferior ends. The impulsion must lose very little of its force by distribution among the branches which combine to form the vascular circle of the skull. The obstacles to circulation, and sources of slackened velocity, are not the same as when the blood is obliged to traverse the capillaries. Therefore, I repeat, I do not think that, on applying two tubes, one to the cardiac, the other to the distal extremity of the carotid, we shall have to note any considerable difference in the level of the two columns of mercury; still we must wait for the decision of experiment. The left carotid has just been laid bare, and the point of the instrument introduced into its cephalic extremity. We shall measure only the force with which the blood tends to advance in the contrary of its natural direction. The anastomoses of the right carotid and vertebral arteries are the chief routes of communication for the liquid. I turn the cock: the mercury oscillates between $60 + 70$, $60 + 65$ millimetres. This is nearly the degree of pressure we should find in the lower end of the vessel, that is, in the direct current of the blood. You perceive, therefore, of what importance it becomes to apply two ligatures, in order to stop the hæmorrhage following division of such an artery as this; the blood would spirt with equal force from both orifices. I now compress the opposite carotid, and we find $50 + 55$, $53 + 58$ millimetres; there is, therefore, a slight diminution of pressure. This shows you that the anastomoses exterior to the skull are not the principal routes by which the blood returns; for, when we arrest the circulation in the vessel which

feeds them, the level of the mercury is but very slightly affected. I no longer compress the right carotid, and the scale in consequence marks 70 + 73, 65 + 68 millimetres. The ascent of the mercury was, as you saw, very sudden. Such is the facility of the vascular communications between the arteries that carry on the cerebral circulation, that the ligature of one of those vessels does not prevent the blood from flowing to, and moving in, its interior. We have on a former occasion learned that the vertebral arteries will furnish the encephalon with blood enough for the support of the hydrodynamic phenomena. Still the singular fact of the supervention of apoplexy after ligature of one of the carotids remains to be explained. In order to judge more accurately of the comparative force with which the blood is moved by the direct impulsion of the heart, and indirectly through the medium of anastomoses, I now apply the instrument to the lower end of the carotid. The mercury gives 60 + 70, 65 + 75 millimetres. This is very nearly the degree of pressure in the upper end.

We will now leave the subject of the venous circulation for the present, and pass to that of the capillary system; and the change is a satisfactory one, for our existing knowledge of the capillary circulation is much more complete and accurate than our acquaintance with the general circulation. In the large vessels we can only judge of the state of the internal currents of blood by the appearance of their walls: according as the latter dilate, contract, become curved, straight, or elongated, we perceive that the former moves with such and such velocity and energy; in fact, it is by induction only that we are enabled to analyse its phenomena of progression. The thickness of the vascular tunics prevents us from directly inspecting the globules of the blood,—an obstacle which does not interfere with their examination in the capillary vessels, as, by the help of the microscope, we are enabled to follow perfectly well the displacements and oscillations of the coloured corpuscule floating in the serosity. The sole difficulty in this branch of the system consists in devising rational explanations for the phenomena observed. I constantly, as you are aware, brought the laws of hydrodynamics to bear, in explaining the circulation in the large trunks; to the same laws I shall refer in our new investigation.

The tenuity of the capillaries is, therefore, favourable, instead of being an obstacle to the examination of the course of the blood in their interior. Injection lends very feeble assistance in the study of the capillary circulation; it can, at the most, do no more than give some anatomical notion of the material arrangements of the vessels themselves. The blood is not the only fluid that moves in the beautiful rete formed by their interlacement: there are certain organs and tissues into the capillary system of which that fluid does not appear to enter at all in the normal state; but, if you push an injection into the vessels, it will penetrate into the canals wherein fluids of another description usually circulate, quite as well as into those in which blood is ordinarily found. Thus, when the material injected is well fitted for the purpose, and cautiously introduced, the serous membranes will become covered with vascular arborisations. Now, the vessels you descry on its surface, when thus injected, were not, during life, traversed by blood; white fluids only were contained in them; but as these hold no opaque granules in suspension, their mode of circulation cannot be accurately followed during life. The lymphatic capillaries have as transparent walls as the sanguineous, and yet what we know of the phenomena occurring in them is exceedingly limited. If you lay bare and examine a serous membrane, you can discover no circulation in the interstices of its tissue; nevertheless, that tissue is principally formed of infinitely minute tubes. You cannot allege that there are no liquid currents in movement; the rapidity with which substances deposited on its surface are absorbed, gives you the proof of the contrary. When this system of white canals, in consequence of some morbid condition, comes into relation with the blood, a fluid hitherto unconnected with them, you may perceive myriads of vessels in the substance of the membrane. It is this change in the colour of the circulating fluid that betrays its presence; so long as it remains diaphonous we are unable to study its course.

I have already adverted slightly to the researches of M. Poiseuille respecting the capillary circulation. I have told you how that observer ascertained that the blood moves in the capillary vessels in the same manner as a liquid in an inert tube; in both cases a motionless stratum adheres to the inner surface by a sort of affinity. Its existence in vegetable tubes, also, has been established by the same experimentalist. Examine the course of the blood in a vessel spacious enough to allow of the passage of several globules abreast, and you will perceive that their velocity of movement is very great in the centre, and less so in the neighbourhood of its walls, while in the stratum of serum they are nearly motionless. In the axis of the vessels the globules are only subjected to a movement of translation; in the neighbourhood of the stratum to one of translation and rotation. The latter is more and more marked the nearer they are to the stratum of serum. The globules that happen to be dashed into it become motionless; those which merely touch it undergo a movement of rotation, as if they had jostled against an undulating surface. This stratum pro-

tects the vessels by means of its immobility, as it prevents the friction of the globules against their walls. It is only where it is in contact with the periphery of the vessel that it is perfectly at rest; the globules move with a progressively increasing velocity, in proportion as they are close to the axis of the vessel. This difference of velocity and movement in the globules placed in the centre, or near the periphery of the vessel, does not exist in its lateral direction only. Those occupying its inferior part are slower in their progress than those situate at its upper end, as may be easily ascertained by examining comparatively those two parts.

The irregularities occurring in the movements of the globules are, therefore, to be ascribed to their relative position to the adhering layer. Thus, let us suppose two globules advancing together with equal rapidity: one of them, jostled by its companion, is driven towards the periphery, its movement slackens, and it remains behind; the other continues on its way. Another jolt, from some other globule, restores the one remaining behind to its former place in the centre, and the moment this occurs it is borne away by the current, and recovers its previous velocity. In other cases a globule gets placed crosswise, so that both its extremities are immersed in the motionless stratum; its movement is thus slackened; others overtake it, press upon, and accumulate behind it; the passage is intercepted, and a sort of dyke opposes the further advance of the globules. Shortly after the globule which has caused all this disorder moves from its anormal position, becomes longitudinal, resumes its motion, and at the same instant all the others reacquire theirs. These agglomerations of globules very rarely occur in cases where the heart retains its full force, and the animal continues strong; they are, consequently, in general, observed only towards the close of an experiment. The existence of a motionless stratum being a continually present and powerful cause of slackened movement in the globules, it is indispensably necessary for the force that moves the blood to have a certain degree of power in order to carry on the capillary circulation. By means of anastomotic communication all these minute canals are made vicarious of each other. When a mechanical obstacle is created in any point, the globules stagnate; the resistance is out of proportion with the motor power, and the liquid remains at rest in its tubes. The central globules are not in the least influenced by the motionless peripheric stratum in the large vessels, in consequence of the distance separating them from it. In the capillary vessels, on the contrary, they are obliged to traverse a mass of serum, of which the central filament alone, if I may so speak, possesses any rapidity of motion. Haller, Spallanzani, and other physiologists, saw the globules advance, retreat, and move in a variety of directions; but these are not, as has been maintained, vital phenomena. The arrangement of the globules among each other, their relation to the motionless stratum of serosity, and other circumstances, give us the key to the comprehension of all those peculiarities. We will continue our examination of this subject in my next lecture.

Gentlemen, in investigating the question of the appearance of albumen in the urine, I, of course, experimented on the largest number of animals I could procure; among the rest was a defibrinised dog. On my submitting his urine to the action of the usual reagent, a large number of flocculi, of albuminous appearance, were thrown down. Now, had I been inclined to give the reins to conjecture, a multitude of hypotheses might have been conceived, in explanation of the presence of albumen in this case; and had I not deliberately considered the matter, and examined the urine analytically, I should have regarded defibrination of the blood among the conditions capable of determining the symptoms of Bright's disease. Nothing, in truth, could have appeared more rational or more probable, considering the results of our previous experiments; nevertheless, close examination showed me that what I had at first taken for flocculi of albumen, resembled laminæ of cholesterine much more strongly. Here, again, is the same appearance, but still more marked, in the urine of another defibrinised dog. The quantity of micaceous particles is really immense, rendering the whole fluid almost solid in appearance; yet I have ascertained, by analysis, that this precipitate is formed neither of cholesterine nor albumen, but of nitrate of urea; and I have further to inform you, that I have, in the same manner, assured myself that the precipitate thrown down in the urine of the animal who perished from forced abstinence consisted of the same salt. For this analytic result I, indeed, prepared you in my last lecture.

The true, or physiological serum, contains (besides the water, albumen, chlorides of potassium and sodium, lactate of soda, carbon, phosphorus, and animal matter found in the serum removed from the body), the globules and coagulable matter of the blood. The latter, the substance well known as the fibrine, exists in the state of solution or suspension, and only becomes manifest to our senses when it changes its nature and solidifies; it then constitutes the clot; and by it, the most living part of the blood, if I may so speak, we will commence our study of the elements of that fluid. To accomplish this perfectly, we should be able, as I have before pointed out to you, to examine it while circulating in

its proper vessels; but, unfortunately, this is impossible, and we are obliged to confine our inquiries respecting it to its solid form. Our recent experiments will prove of considerable use to me in this respect, for I am not aware that any other person has isolated the fibrine from the rest of the blood, at the moment of its assuming a distinct form, and organising into a net-work, to form the clot. Physiologists have hitherto been contented with beating up arterial or venous blood immediately after it had been received into a vessel, by which process numerous filaments became attached to the rods used for the purpose. These filaments were, in the main, composed of fibrine, but were also invariably coloured red, from the adhesion of the globules to them; the latter were only separable from them, so as to render the fibrine nearly colourless, by repeated washing. This is the method employed by butchers to prevent the blood from coagulating.

Another manner of procuring fibrine consists in allowing the blood to coagulate, submitting the clot to pressure in a linen cloth, and then washing it, so as to remove the colouring matter. But by this process we simply obtain a compact mass of fibrine, which is only available for the determination of the specific gravity and chemical composition of that substance; no one ever seriously attempted to grapple with the question of its organisation when in this state. Both these methods, besides the expense of time and trouble they demand, are therefore, radically imperfect. Had the nature of the blood been better understood, and had medical men not obstinately persisted in regarding the *buff* as a particular morbid element, they might have availed themselves, without any trouble, of a means of studying that substance. Thus theories, having no solid basis to rest upon, are perpetually, and even in the most trifling details, interfering with the progress of the science. When we examine the blood in circulation in a living animal, we perceive nothing but a liquid, bearing along with it the globular bodies to which I have so frequently adverted. No trace of fibrine is to be discerned, and hence the impossibility of studying it in the state of solution or suspension. As I have not, by my own researches, added anything to what is known through chemical analysis, I shall not trouble you with an enumeration of the proportions of carbon, oxygen, hydrogen, and nitrogen, entering into its composition; these you will find in the most recent works on the subject. To the question of its organisation I wish more particularly to direct your attention. Some time past I had remarked, that on receiving blood into a vessel containing sugar and water, the fibrine separated from the globules, and formed a sort of lamellar tissue, with very delicate meshes, resembling in structure the organised membranes of the animal economy. This was too remarkable a fact not to excite my curiosity; I repeated the experiment, and the same phenomenon invariably made its appearance; you see on the table before me numerous specimens of it. All these vessels, which I also used in my last lecture, contain fibrine, more or less distinctly organised. One of the most remarkable circumstances connected with the formation of these net-works, is the fact that the presence of much liquid seems to offer no opposition thereto; thus, whatever be the quantity of water you add to the blood, provided it contain the least share of fibrine, that fibrine will detach itself from the rest of the fluid in the shape of long filaments, which contract adhesions with the inner surface of the vessel, interlace, become agglutinated together, and so form an undulatory net-work, modelled to the shape of, and filling the whole interior of, the vessel. Here we have ten grammes weight of blood and sixty of water, yet you can distinctly perceive the parenchyma that has been formed. I propose that these cellular and filamentous, scarcely visible masses be termed *nebulous clots;* for they really resemble a transparent cloud, or light thin froth. I examined a small lamella of this clot under the microscope with the following results: I first saw an infinite quantity of minute, sinuous, undulated lines, as it were, festooned, and placed side by side; here and there were some small globules, of less size than those of the blood, the nature of which I was unable to determine; next large septa, which divided the mass, and interlaced in a multitude of directions. Many observers have advanced as their belief that the fibrine is formed of globules; I must confess that I consider them in error, for though I have examined that substance with all the attention of which I am capable, I have never succeeded in discovering in it the slightest trace of a globular structure; the notion should, therefore, I think, until at least some new evidence be adduced in its support, be rejected as unfounded.

I have another very curious phenomenon to describe to you, connected with the same subject. I had removed one of these *nebulous clots* from a mass of liquid, and sat it aside to examine its structure more closely. I was on the point of throwing away the remaining contents of the glass vessel, when I perceived that a new clot, of still more cloudy appearance than the first, had formed in the interim; to this I have given the name of *clot of secondary formation.* By this unexpected occurrence I learned that part of the fibrine remained in solution in the liquid, and became organised on the removal of the first clot. I beg to add, that though this formation takes place in sugar and water, it does not do so in other vehi-

cles, as, for example, a solution of subcarbonate of soda; it is necessary for its appearance that the fibrine be in a condition capable of coagulating. Every one of the mixtures of blood and sugar and water on the table produced clots, more or less voluminous, according to the predominance of the fibrine. Here are some more solid than those of which we have been speaking; they form numerous cells in the interior of the vessel, and the walls of those cells give off filaments, traversing their cavities in every possible direction. These cells are filled with the liquid; and it is very probable that the curious fact we observed of blood, to which we have added a large quantity of water, solidifying in such manner as to allow of the escape of only a small proportion of it, is to be explained in this manner.

But to pursue our analysis, physiologically speaking, of the coagulum: underneath the *nebulous clot* you perceive a more dense stratum; this is nothing more than what has been termed by pathologists the *buff*. Its composition is the same as that of the *nebulous clot*, but its texture is more compact, and its cells contain less fluid. This, which may now appear to you little more than a mere assertion, I trust I shall rigorously demonstrate to be the truth in due season; meanwhile let me assure you that the *buff* has no real claim to the importance usually attached to it; it is nothing more than so much fibrine, as I have already mentioned to you, separated from the globules during the process of coagulation. I have no doubt but that, in the course of a few years, medical men will smile at the recollection of the horror with which that terrible phantom was once regarded. The blood of some animals, the horse among others, contains a very large quantity in the normal state; and veterinary practitioners, herein much more sage than ourselves, far from looking on this mere physical effect as a pathological sign, have termed it simply the *white clot*. Nor is it in the blood only that fibrine is discovered in the free state; none of you are unaware that it enters into the composition of the false membranes developed in certain affections; that it is occasionally met with in collections of fluid that accumulate in the cavity of the pericardium, or pleura; and that it appears in its most evident form in the pseudo-membranes of adhesions formed between the edges of a solution of continuity, or after loss of substance.

There is another property connected with the formation of the clot, of which I have so far only spoken to you incidentally, I mean the retraction that takes place in it, to a greater or less extent, according to the conditions in which it is placed, conditions respecting which very little is known. Nevertheless, it is right to note this phenomenon when it occurs; and here is an excellent example of it in the clot formed in a solution of hydrochlorate of soda. I showed it to you in my last lecture; it then filled the whole vessel, it has now lost one-third of its former size by retraction. Remark, too, the extraordinary circumstance, that the most trifling movement of the vessel seems to communicate myriads of tremulous motions to the mass, which spread, by undulations, through its entire substance, and give it the appearance of one of the mollusca, moving itself by a series of vibratile contractions.

The word fibrine is of recent introduction, at least I do not recollect to have met with it in the writings of any author previous to Lavoisier; the name was given to the principle of the blood under consideration from the presumed identity of its composition with the muscular fibre, which, according to some physiologists, consists of filaments of fibrine. I have already mentioned to you an important fact, showing the fallacy of this notion, but shall, at a future time, enter more fully into the subject. MM. Prèvot and Dumas have advanced, in an essay published in their joint names, that the coagulum is formed by the adhesion of the globules to each other. This theory of its formation has been generally adopted, and many chemists and physiologists of the present day subscribe to its correctness. This error does not surprise me, for as serious ones are committed in less difficult investigations; and, in truth, when we examine an ordinary clot it appears quite clear that the globules are present, adhering to the filaments of fibrine, and appearing to form an integral part of it. But this is, after all, a mere appearance; if you wash the clot sufficiently the globules are carried away, or dissolved, according to the nature of the liquid used for the purpose, and the fibrine makes its appearance with all its true characters. This is a fundamental fact, on which it is most important to have correct ideas; the globules are, it is true, present in the coagulum formed in blood drawn from a vein, but not as an integral part of it. I am the more anxious to impress this on your minds, as my conviction respecting it differs from that of observers of the highest order. One among them, whose immense and well-deserved reputation gives great weight to every opinion he originates or espouses, believes not only that the globules enter into the organisation of the clot, but further, that when they have once formed part of it, their structure changes if they be again separated from it.

I really cannot understand how Berzelius, the glory of modern chemistry, came to adopt such an opinion as this; one which falls to the ground before a most simple experiment. Take some blood, beat it up;

you separate the fibrine thereby, and there remains a liquid, incapable of coagulating, composed of the globules and serum; next examine these globules with the microscope, and you discover in them the same lenticular form, the same investment, the same central point, and the same colour as before. No change has, therefore, taken place in them; you find them, after the operation, exactly what they were before it. Now this point is really of great interest; all my experiments go to prove that, strictly speaking, we might understand the possibility of life in blood composed of serum and fibrine only; as to the globules, nothing positive is known respecting their uses; and, for my part, I know no other kind of utility they possess beyond that of facilitating the microscopical investigation of the blood. Nevertheless, as they do exist, and consequently there must be some reason for, and usefulness in, their presence, in the midst of the vital fluid, they shall, in due season, receive their share of our attention. The phenomenon of the separation of the serum from the fibrine has been a favourite subject of inquiry with physiologists, but as they were unable to obtain the fibrine in a solid state without the globules, they thought that the latter enjoyed equally the property of coagulating. Burdach and Müller, however, appear to have attacked the question in the same manner as I now do. While engaged with experiments on the blood of frogs (the globules of which are, as you are aware, of a different form, and larger than those of the mammalia), they had remarked, that when that blood was laid on a filter a certain quantity of a transparent, colourless fluid passed through, leaving behind it a small reddish mass; the latter consisted of the globules; the separated fluid soon coagulated. The filter they employed was made of rabbit's bladder, and the process was simple and ingenious, but little fibrine was obtained by it. The method of procuring it, by mixing blood with sugar and water, is more successful in its results. The species of membrane to which I have already called your attention, and which you see floating in these vessels, resembles strongly those lining the uterus during the early period after fecundation. At that period the uterus undergoes a particular modification: and I am inclined to conjecture that the distension its tissue undergoes dilates the orifices of the vessels terminating in it; the blood is distributed to it in greater quantity than usual, its fibrine transudes on the fœtal surface of the organ, and forms the membrane placed between it and the ovum. From what we know of the properties of the fibrine in the formation of the cicatrices of wounds, I should incline to believe that the fœtal membranes really become organised, though I have no decided opinion on the question.

In the *nebulous clot* I now shew you you will perceive a certain vascularity. The occurrence of this vascularity did not escape the genius of John Hunter, who rather divined, than discovered by research, the importance of these studies. I saw, indeed, in his anatomical museum in England, a specimen of a clot presenting an appearance of real organisation. Here is another example of coagulated human blood, in which a very distinct organised frame-work is visible; this fibrine is colourless too, which results from our having employed sugar and water, and so prevented the solution of the globules. Had we mixed this blood with pure water there would still have been a very distinct clot; but, as the water would have dissolved the globules, the red colouration of the liquid would have rendered it impossible to scrutinise the structure of the coagulum properly; hence, according as we modify the nature of the liquid in which we place the blood, the results vary. I trust these experiments will lead to some valuable inferences; so far they are certainly very curious. Here, again, ten parts of dog's blood have been mixed with sixty of human serum; and observe the singular production to which the mixture has given rise. We have a cylinder widened at both ends, these being formed of areolar tissue, while the colouring matter is deposited in the centre; the *nebulous clot*, instead of appearing at the upper part, in its usual position, is seen at the bottom of the vessel, while the *cellular clot* is placed superiorly; the deep red substance you see in the middle constitutes the ordinary clot of pathologists; here and there are scattered membraniform formations, which connect the cylinder with the surface of the vessel. I must try whether a variation in the proportion of water will cause a modification in the arrangement of the fibrine, for I am of opinion—this I confess to be a simple conjecture however—that it does not solidify in the same manner in different acute affections.

Physiologists have exerted themselves much in endeavouring to ascertain whether the coagulation of the fibrine is a physical phenomenon, or one dependant on vitality. Certain substances possess the property of solidifying, such as peptic acid and gelatine; the latter, mixed with one hundred times its volume of water, forms into a mass when submitted to a temperature gradually cooled down. It is clear that no vital influence is in action in this instance; the serum also solidifies when brought into contact with acetic acid; but this is simply the result of the chemical action of the acid on albumen; but the case is different with fibrine. Besides, when albumen and serum are solidified by heat, or a chemical agent, no traces of arborisation, which are, indeed, distinctive of fibrine, are detected. For my own part I do not attach such great importance

to its being of chemical, physical, or vital nature; nevertheless, it is well I should remind you of a fact, which, if it have been correctly observed, militates against its being of the latter character. It is this: if we submit blood to the action of intense cold before coagulation it congeals; and if we then remove it from the influence of the low temperature it becomes liquid again, and next coagulates. I confess that this is one of the strongest objections that can be brought against the vital organisation of the fibrine, for coagulation usually destroys life in all animals. Nevertheless, as I only give you this fact on hearsay evidence, I draw no conclusion on the matter until I have verified it myself. I will expose some blood to the action of a frigorific mixture immediately after the lecture, and I hope to have something of a positive nature to communicate to you on the subject when we meet again.

If, instead of a saccharine solution, we add carbonate of soda, in solution, to the blood, in the proportion of sixty-three of the former to seven of the latter, the blood so treated remains liquid; it even undergoes a commencement of decomposition; no traces of fibrine or globules are to be found, for both are soluble in the alkali. Here, then, is a substance which evidently opposes the coagulation of the blood; and as the property possessed by that fluid of forming into a mass, exercises a powerful influence on our organs, any agent interfering with it must prove eminently prejudical to the exercise of those functions. I, consequently, propose to undertake a series of experiments in which we will pass in review the action of the chief medicinal and alimentary substances on the blood.

I have made a mixture of ten parts of blood and sixty of a solution of hydrochlorate of ammonia in this vessel. The result is the formation of a clot filling the whole interior of the vessel, but of slight powers of resistance, tearing, with facility, and retaining much water in its meshes. Here is another mixture of the same substance, but in different proportions: sixty-five of the saline solution to five of blood. In this mixture the colouring matter has been dissolved, and a sort of partial coagulation takes place; the clots resemble the current jelly *(gelée de groseilles)* of pathologists. The same substance will, therefore, produce different effects, according to the quantity in which it is employed. But do not, therefore, suppose that these researches are devoid of utility; they are intimately connected with pathology. Many and many a time you must have had occasion to observe at post-mortem examinations blood and clots resembling those now shown you. You took note, no doubt, of this character of the blood, but did you inquire into its cause? Probably not; yet it is this which it would first have come into your minds to do if you had not been pre-occupied by the systems in vogue, which attribute everything to the solids, and refuse the liquids all participation in the production of morbid alterations. Here is a mixture of two parts of venous blood and sixty-eight of an aqueous solution of hydrochlorate of soda; it presents a scarlet-coloured clot; it is also distinctly organised, and is the seat of a kind of peristaltic motion, due to its extreme elasticity. This other mixture of sixty parts of the same saline solution, and ten parts of blood, is wholly solidified; is is firm, resisting, and, like the former, of arterial hue. It results, from all this, that certain bodies diminish, or destroy completely, the power possessed by the blood of forming into a mass, while others promote its tendency to coagulation.

OBSERVATIONS
ON THE
FALLACY OF THE INDIVIDUAL SIGNS
AND
SYMPTOMS OF PREGNANCY.

By J. Reid, *M.D., Consulting Physician-Accoucheur to the St. Giles's Infirmary.*

There are some complaints the symptoms of which so closely simulate those of pregnancy, that a difficulty is *occasionally* found in giving a decided opinion, even by those who have bestowed great attention on the subject, and have the experience of years to guide them. In illegitimate pregnancy we have the greatest difficulties to contend with; for, far from the female affording us any aid in forming our opinion, she says everything calculated to mislead us, and to such an extent is this sometimes carried, that labour pains themselves are attempted to be represented as the effects of colic, and the cries of the newly-born child are even met by the still firm denial of the mother as to her pregnancy.

Dr. Hamilton, of Edinburgh, asserts confidently, that "both in the early and latter months of pregnancy, there are *invariable* signs marking that condition of the system, and that for the last thirty years he has not met with a case where he could not ascertain pregnancy after the fifth month." This opinion is quite at variance with that entertained generally by those professing to be conversant wtth the subject, and, contrary to that expressed by Smellie, Baudelocque, Gooch, and other eminent authors.

In the present memoir I shall omit the *general* symptoms of pregnancy, and some peculiar ones which *occasionally* make the female aware of her condition at a very early period, and proceed at once to consider the individual signs, commencing with

Changes in the Appearance of the Breasts.

The Enlargement of the Breasts, if taken in conjunction with the other signs of pregnancy, is of some relative value, but by no means one on which we can at all depend. It frequently occurs not only *during the menstrual period*, but likewise when this discharge does not take place at the proper time, owing to functional derangement. It may be caused by several morbid conditions of the uterus.

In some females the breasts enlarge immediately after the marriage, *without* pregnancy being the cause.

Again, in many women of delicate and spare form, the mammæ undergo no visible change as to size, even up to the time of parturition; while in those who are of a robust and very full form, there is as much difficulty in distinguishing any difference. At the age of 45, or about that period when the gradual disappearance of the catamenial discharge generally occurs, it is not at all an unusual circumstance for the breasts to become more fully developed, attended with pain, and should the female have married at this age, she may be easily deceived by the coincidence of these signs.

Areola.—A very wide difference of opinion exists among obstetric authors of the greatest celebrity as to the value of this sign of pregnancy, and on this account, perhaps, it may not be improper to place on record more fully than I should otherwise have done, the individual opinions of some of them. Denman affirmed that it might be found in many complaints resembling pregnancy, and not always in cases of the latter.

Smellie, curiously enough, describes the areola as *only* perceived in pregnancy, but still thinks that even in first pregnancies it is an uncertain sign. W. Hunter, again, attached great faith to this appearance; he asserted that he could always judge by it as to pregnancy; and a remarkable instance of his discovering the existence of the latter in a subject, by the areola alone (although the hymen was entire), has been cited by many authors. Dr. Blundell, who has studied the changes of the areola with a good deal of attention, says, "that when these rise to the highest point (or are very distinctly marked) they ought to have great weight, at least, in a first pregnancy, but when only in a secondary degree they are of little value; when a woman has already borne children, indications of the breasts cannot be safely relied on at all." An American author, Dr. Dewees, thinks the areola equivocal in any but a first pregnancy, but in this case he places great dependance on it, though the absence of the areola does not prove non-pregnancy. Dr. D. Davis says it is the best single evidence we possess, but in women of very fair complexion it is *scarcely observable*; and in others it sometimes does not make its appearance till after the first pregnancy. Gooch believed that the dark colour of the areola *rarely* depends on other causes than pregnancy, and that when it exists it may generally be looked on as a sign that the patient is pregnant, or *has been*. He immediately, however, gives two cases of young married women whom he had seen *in one week*, in whom the areola was dark, and on this account principally they had made preparations for their accouchement, though neither of them was pregnant. The above admission of Dr. Gooch does not satisfy the mind of Dr. Hamilton, of Edinburgh, as the former has said, likewise, that in women of very fair complexion it is often difficult to perceive an areola, and that in brunettes, after first pregnancy, the areola is no guide. After denying that Gooch's facts are founded on accurate observation, he proceeds to state, "that in fair women, previous to impregnation, there is no perceptible discolouration around the nipple, excepting during menstruation, when there is a slightly-marked circle, of a blueish hue, like milk and water. When they become pregnant this circle is again apparent, towards the end of the third month, and after the fifth it is of a brownish tint. But women of *dark* complexion have the nipple, in the unimpregnated state, surrounded by a *brownish* circle, which, after the third month of pregnancy, gradually becomes darker, so as to resemble old mahogany. *Swarthy* women, again, have naturally, in the virgin state, a mahogany-coloured areola, which, in the progress of pregnancy, is gradually converted into a *black purple*." It is Dr. H.'s decided conviction, that the areola can always be distinguished by an experienced eye, and that, as a general rule, the breadth of the discoloured ring increasing as pregnancy advances, may be trusted to, though he has seen a few remarkable exceptions.

Breasts.

Dr. Hamilton places his principal reliance, however, on "a certain degree of turgescence on the surface of the discoloured ring, which becomes more and more distinct towards the latter end of pregnancy." In women, he says, who have already borne children, and who retain a brown or dark areola when not pregnant, this turgescence is not to be recognised; but during pregnancy it returns, is in most cases visible to the naked eye, and can in *all* cases be detected by a magnifying glass, when the elevation of its outer surface may be seen distinctly. On the whole, Dr. H. thinks that when suppression of the catamenia is followed by the before-mentioned changes in the mammæ, *there can be no doubt* of the nature of the case.

Dr. Montgomery, of Dublin, has written by far the best treatise on this subject which

we possess, and although I have to premise that, as the result of my own researches on the subject, I do not find that I can place such entire confidence in the signs which he has so ably described, I feel no hesitation in admitting that, as a general rule, I have found them perfectly correct, though the exceptions are by far more numerous than he allows of. Dr. Montgomery rather disagrees with Dr. Hamilton as to the value of the dark circle. He says, "most persons attend only to the colour, which, in his opinion, is the one, of all others, most liable to uncertainty." He particularly describes a soft and moist state of the integument near the nipple, presenting an emphysematous appearance. He has recognised this as early as the end of the second month, and during the progress of the next two months the areolar changes are in general perfected. At the end of the fourth month the circle varies from one inch to an inch and a half in diameter. He has seen it at the time of labour three inches in diameter. The nipple appears turgid and prominent, and the disk at its base is studded with glandular follicles, which likewise are scattered over the other parts of the areola.

In women who have dark hair and eyes, numerous round spots, or small mottled patches, of a whitish colour, are scattered for an inch or more round the outer part of the areola, as if the colour had been discharged by a shower of drops on the part; this Dr. Montgomery has not seen earlier than the fifth month. About the sixth month a number of shining, whitish, almost silvery lines, like cracks, are seen, but as these afterwards remain permanent, they will not serve as a diagnostic mark in a future pregnancy. In nurses the characters of the areola are kept up, and *continue* in a state of considerable perfection.

In venturing to differ in opinion with authors of such experience and authority as Drs. Hamilton and Montgomery, as to the certainty of the signs of pregnancy indicated by the mammary changes so fully described, I do not intend to deny that were I to meet with all, or greater part of them, in examining the breast of any female, I should feel quite inclined to admit that they afforded a very strong presumptive evidence of a present or former pregnancy; but I do feel justified in asserting, from experience, that there are *numerous* cases of pregnancy where these signs are not to be distinguished at all, as differing from the mammæ of those who are not pregnant, and that in the latter class some of the more prominent symptoms even, are occasionally to be met with. I have long since made up my own mind not to trust to *simple darkness of an areola*, even in those of fair complexion, as I have been several times deceived by what I was formerly taught to believe almost an unerring sign; and I am persuaded that the majority of obstetric practitioners have occasionally found this to be the case with themselves. Even with the precise definition of Dr. Hamilton, as to the *three* complexions of different women, he admits that as there is *every shade* between these, so there is a corresponding shade of areola in those even who are not pregnant. Now, as the areola which, in one class, is to assure us that they are not pregnant, is the same which tells us in another, that they are, it will, I conceive, in these different gradations, be extremely difficult for us to hit upon the precise point in them which is to guide us as to the presence or non-existence of pregnancy.

In the course of the two last years, especially, I have had numerous opportunities of observing that, in young females who have scarcely arrived at puberty, there is often a brown disk round the nipple equal in diameter and intensity to *some* which I have seen in pregnant women; but in these cases I have never discovered the presence of follicles. In single females of more advanced age, however, I have occasionally met with the areola distinct, and the follicles as much so. In a remarkable case of an unmarried lady, æt. 38, who was suffering from chronic tumour of the left breast, follicles might be seen, though the patient certainly was not pregnant. It is only about three months since she first consulted me, and on examining the nipple and surrounding parts I must confess I was a little startled at observing not only the prominence of the follicles, but the identical emphysematous and moist appearance about them so well described by Montgomery. As I have had frequent occasions of viewing the part since that time, I have remarked that not only does the depth of colour in the areola itself vary at different times, but likewise the prominence and moist appearance of the follicles and surrounding part.

Another effect on the areola from exposure of the breast to the air, which I have frequently observed, is, that in a few seconds a tolerably broad disk will be visibly corrugated, and reduced materially in diameter; so that in making an examination it will be prudent not to allow of this exposure for some time previous to our visit.

Dr. Montgomery does not assert that the glandular follicles are *always* to be seen in conjunction with the areola, in pregnancy; or that the areola itself *always* changes in characters; but he has never seen an instance of their development, when accompanying the *other* changes, *without the concurrence* of pregnancy.

In giving our opinions, then, as to the presence or non-presence of pregnancy, founded on the examination of the breasts alone, I think we should be extremely guarded. If we are to expect such changes as those described in the plates of Dr. Mont-

gomery's work, I am afraid we shall be deceived in the majority of cases; for although those appearances are familiar to me, as having observed them in *some* females of dark complexion, I do not think they will be met with in one case out of fifty, at least to the extent represented by the delineations. In this I am borne out by the opinions of several eminent obstetric lecturers whom I have questioned on this point. Whilst engaged in writing this paper I have had opportunities of viewing the mammæ of two females who have already borne children, in whom the follicles are as distinct and moist as I have ever observed them to be in pregnant women, though in these no pregnancy exists. On the other hand, I last week had occasion to examine, in our lying-in ward, the breast of a woman whose labour was just commencing (and who was afterwards delivered of a *live* child); here the mamma was quite flaccid, and although the patient was of a moderately dark complexion, there was a very light shrivelled areola (she had previously borne children), and it was only by distending this with the fingers that I was enabled to perceive the very indistinct, *dry*, and flattened follicles.

On the whole, I believe the appearances of the mamma, when presenting the signs before described, to a certain extent, to be strongly confirmatory as evidence of pregnancy, if they be combined with all the other symptoms, but that there are numerous cases in which they will be found absent where pregnancy exists, and a smaller number, in which they will be discovered in women who are not *enceinte*. I feel bound to coincide with the reviewer of Dr. Hamilton's work in Dr. Johnson's journal, who states,—"Our own experience, and we may add that of almost all the latest and best writers of midwifery and medical jurisprudence, do not certainly warrant that unqualified and unequivocal reliance on the changes of the areola, as a sign of early pregnancy, which Dr. Hamilton puts on them."

The Secretion of Milk is a very uncertain sign indeed of pregnancy, as there are numerous cases on record in which very young girls (æt. 8), and old women (even up to the age of 70) have been enabled to suckle infants, so plentiful did the secretion become. Nay, on unquestionable authority, we have cases of men, even, suckling their infants, and proving very good substitutes for the other sex. Humboldt, Captain Franklin, and the Bishop of Cork, have each related instances of this fact, and they may be found in the "Personal Narrative." Blumenbach explains it by the anastomosis of the epigastric and mammary arteries existing in males as well as in females.

Arrested Catamenial Discharge.

Dr. Hamilton has no hesitation in asserting that this *invariably* attends pregnancy, even during the early months, and that those cases which have been adduced to prove the contrary, will always be found to present a difference as to quantity, regular time of recurrence or duration, and quality. Denman, Burns, and D. Davis, support this assertion. Against it are arrayed—Heberden, Deventer, Haller, Baudelocque, Velpeau, Capuron, Hosack, Dewees, Gooch, Blundell, Montgomery, and many others. No doubt can exist as to the fact of a monthly discharge occurring often during the early months of pregnancy, and sometimes even up to the latest period. Whether this be really menstrual or not, matters little, if the resemblance be so close as to render the female unable to distinguish between them. Besides, we find many women whose catamenia (when not pregnant), are deficient in quantity, duration, and regularity of occurrence.

Cases occur in which a profuse sanguineous discharge is occasionally a sign of pregnancy to some females; whilst in others a strong leucorrhœa of four or five days duration takes its place.

It must be borne in mind that in some cases of illegitimate pregnancy an attempt has been made to imitate the catamenial discharge. Notwithstanding the arguments of Dr. Hamilton and others, I still feel that "the absence of menstruation does not hold good as a symptom of pregnancy, nor does its presence ensure exemption from it."

Quickening

Is one of the least to be relied on of all the signs. It generally takes place at the fourth month. Dr. Hamilton knew of one case where it occurred at the eleventh week; and I have known five in which (judging from the time of their accouchement, and the statements of the patients) it must have taken place at the end of the twelfth week. Sixteen weeks is the usual time, however. Numerous cases are related by La Motte, Baudelocque, Mauriceux, Desormeaux, Gooch, Montgomery, Campbell, and others, in which the mother has never perceived any motion of the child throughout pregnancy, though it was afterwards born *alive*. Dr. Hamilton sums up his remarks on this subject by saying, that he holds all these alleged cases to be the *offspring of prejudice and credulity*. In a case of my own, the patient merely felt a slight fluttering about *once a day* up to the eighth month, though the child was born alive.

The Movements of the Child are said often to be felt by those who afterwards proved not to be pregnant. Queen Mary (according to Hume) *felt the babe leap in her womb*, on the Pope's legate being introduced to her, though it proved afterwards that it was nothing but dropsy.

The following cases are simply to show how females themselves may be mistaken,

and that however accoucheurs may occasionally err in those of others, the accoucheuse may be deceived in her own:—

Case 1.—Jan. 15, 1838. Mrs. F., midwife, æt. 30, has had fourteen children; her catamenial discharge had stopped for the last five months, but has again appeared this week, which circumstance was the cause of her consulting me on the subject. Her size has gradually increased; the breasts have become much more full, and from one of them there has been a free exudation of what she says is milk; she has suffered from morning sickness and the other general symptoms of pregnancy, and as is always the case with her in that state, one of her legs has much swollen. At the end of the third month she had an attack of fever, which lasted nearly six weeks, and she quickened, as she thought, during this time; her size still continued to increase, and the apparent motions of the infant became stronger and more frequent, as distinctly as she had felt any of her other children at the same period. All her symptoms, externally, were those of pregnancy, but internal examination proved that she was not in that state.

The following was related to me, as her own case, by the most intelligent midwife I am acquainted with:—

Case 2.—Mrs. A., midwife, æt. 20 (at that time practising as midwife in one of the West Indian islands), and mother of three children, found the size of her abdomen and breasts gradually increase, the former to the full extent of ordinary pregnancy; the catamenial discharge had completely stopped, and all the ordinary symptoms of pregnancy followed; she could, after a certain period, distinctly feel the movements of the infant, not only internally, but could trace with her hand, externally, through the parietes, its head rolling from one side to the other occasionally. She went to the term of nine months and a half, when a show took place, which was soon followed by pains, and a profuse sanguineous discharge, continuing through the night. The medical attendant had been sent for, and remained in the next room during this time, as she was unwilling to allow of an examination until the pains became forcing; at length it was made; it was discovered that she was not pregnant, and next day the abdomen was reduced to its natural size; the discharge continued, to a trifling extent, for a week, but no substance passed from her during the whole time; six weeks after this she really became pregnant.

Case 3.—I was requested by a midwife to see a patient of hers, æt. 29 (in labour), at Castle-street, Long-acre, as she had been called up to her three successive nights, owing to her having strong pains, which, however, always wore off towards morning. The os uteri, the midwife informed me, was not at all dilated, but the patient, who had had eight children previously, thought she was about eight months advanced in pregnancy. On my arrival I found, in answer to my inquiries, that seven months since she was much troubled by sickness and retchings, accompanied by dislike of food, and the other usual symptoms of early pregnancy; the catamenia did not stop altogether, but there was an occasional discharge, irregular, both as to time and quantity, and attended with more pain than usual; she had gradually increased in size about the abdomen, which became tense, and about three months and a half after the probable time of conception, she first thought she perceived the movement of the fœtus, which was not frequent or strong, but all her sensations were such as she had felt in her other pregnancies.

On examining this patient I found the breasts not very full, but there was an areola; the abdomen was tense, and felt like the pregnant one, but the navel was depressed. By applying the ear to the abdomen nothing was heard but a flatulent murmur, and, on examination, the os and cervix uteri were evidently those of an unimpregnated womb; so the midwife was disappointed of her fee, and the baby-clothes put back into the drawers again.

External Examination of Abdomen.

The augmentation in the size of the womb is seldom to be felt until the tenth week. Dr. Hamilton is astonished that practitioners of midwifery, of established character, should think, as they have done, that this, at the latter months of pregnancy, should ever be attended with *doubtful* results; and he affirms that the movements of the infant can be always distinguished by an attentive practitioner. There are, however, many circumstances which embarrass him in making this examination. It is well known that Louder and several other eminent men minutely examined the abdomen of a woman who could simulate the movements of a fœtus so perfectly that they all felt obliged to satisfy themselves by other means that she was really *not pregnant*.

The Projection of the Umbilicus is a valuable *negative* sign in the latter months of supposed pregnancy, for should this be absent, it is tolerably certain that the woman is at least not so far advanced as she suspects.

Examination per Vaginam.

This has been termed by Velpeau the "boussole de l'accoucheur." It is of no avail, however, during the earlier months. Dr. W. Hunter did not like to decide by it until the expiration of the seventh month, and even then, it requires long habit and experience to become expert in.

Describing the various conditions of the os and cervix uteri at the different advanced

stages of pregnancy, I would advert to ballottement *en passant*, and also pass over in silence auscultation as applied to the subject of pregnancy, as Dr. Williams, who is so well qualified, had made some remarks on it last session. M. Jacquemin has proposed a new test of pregnancy, viz. that during this state there is a deep violet tint, like lees of wine, over the whole mucous membrane of the vagina, and this sign, he says, *never failed him*. Dr. Klüge, of Berlin, has always found it a sure diagnostic mark, whilst Dr. Sommer and Duchâtelet confirm this statement. In practice, however, this sign cannot *often* be of any avail to us, as it is only under extraordinary circumstances that we could propose its adoption; but in these it ought (from the high authority which recommends it) to have some weight.

The Presence of the Hymen in unmarried females is not of such value in deciding on this subject, as might at first appear. Many cases have been related in which *the birth of the child* was retarded by *its presence*. In some anomalous cases of pregnancy no vagina has been discovered to exist. Duchâtelet who, in his investigation, examined 4500 females, says it is even impossible by any ocular proofs to determine as to virginity, as some women who had been prostitutes for twenty years, presented as likely signs of it as those who really retained it.

In concluding these remarks on the diagnostic symptoms of pregnancy, I have to apologise for the length to which they have unavoidably reached, although some important remarks connected with the subject have been omitted.

ESSAYS IN SURGERY.

By J. M. Coley, *Esq., Senior Surgeon to the Bridgnorth and South Salop Infirmary.*

CANCROIDE, KELOIDE, OR SCIRRHOIDE.

This singular disease was first described by Alibert, under the name of *cancroide*, a corrupt term, derived from the Latin word *cancer*, and the Greek word Εἶδομαι, *to be made to resemble;* which should have been *karkinoide*, from Καρκινος, *a cancer*, and Εἶδομαι. The word *keloide*, which is derived from Κηλη, *a tumour*, and Εἶδομαι has been adopted by Biett, in his clinical lectures on the diseases of the skin. *Scirrhoide*, derived from Σκιρρος and Εἶδομαι, signifies *a malady resembling schirrus*.

It begins with slight elevations in the skin, which, increasing in size, terminate in tumefactions of an oval shape, with a depression in the middle, or in angular or longitudinal tumours, resembling the cicatrices of deep burns; they are hard and gristly in feel and appearance, and the epidermis covering them is attenuated and wrinkled; their colour is either pale-white, pink, deep red, or brown, and they project one or two lines above the level of the adjoining skin, their circumference being more elevated than their centre; their diameter seldom exceeds a few lines, but occasionally extends to half an inch or an inch, and in some instances to five or six inches, or more. In general this disease is accompanied with deep, shooting pain; its progress is very slow, and it sometimes becomes absorbed, leaving a white, hard cicatrix behind it, resembling a part which has been tied with a string. It has been met with in various parts of the body, but most frequently on the extremities and front of the chest. In one instance I have observed it on the head; and in two cases it has terminated in extensive scrofulous ulcerations.

Diagnosis.—According to my own observation, this complaint resembles cancer only in appearance: it may be distinguished from the latter by the comparatively sudden manner in which it makes its attack; by its not invading the absorbent system; and by the absence of the cancerous diathesis. There is certainly one form of carcinoma greatly resembling this disease. This is the angular scirrhus found in the female breast in advanced life, presenting an irregular puckering of the integuments, of a pale complexion, like the cicatrix following a clumsy attempt to unite a lacerated wound by the first intention. In this case the indurated part, instead of being elevated above the surrounding skin, as in keloide, is depressed; the disease is of many years' duration and incurable, and is accompanied in its progress with scirrhous tubercles and with cancerous cachexy, which, whether the ulcerative process may occur in the part or not, ultimately destroys the patient. Tubercles are sometimes found in extensive scirrhoids. These, however, are not situated in the absorbents, do not possess the cartilaginous feel observed in carcinoma, and under proper treatment they disappear.

Pathology.—The disease consists of a deposit of tubercular matter in the cellular membrane, immediately subjacent to the adherent surface of the cutis vera. My reasons for entertaining this opinion are the following:—The white, or straw-coloured, or pink complexion of the elevations, which resemble other scrofulous tubercles; the solidity of the tumours; the removal of the specific deposit by absorption; the puckering and inversion of the integuments observable after the subsidence of the disease, which resemble those appearances found in the lungs after the thinner portions of the tubercular deposit has been absorbed; and the scrofulous diathesis manifested in those afflicted with this rare complaint who have come under my notice. This deposit of tubercular matter under the skin is probably the result of a previous inflammatory

condition of the blood, which, in scrofulous constitutions, is prone to generate that morbid product. In one instance, where incipient pulmonary phthisis was arrested by timely depletion, I discovered a small mass of tubercular matter in the blood, ready to be deposited, and the same fact has been noticed by Magendie and others.

The pain and smarting accompanying the growth of these indurations, seem to proceed from the distention of the skin.

Treatment.—The most efficacious mode of treatment consists in the internal and external use of iodine. Various other remedies have been tried in vain: as local bleeding, blistering, mercury, compression, sedative plasters, and extirpation. In a case which occurred in the practice of Dr. Warren, surgeon to the Massachusetts General Hospital, the morbid parts were twice extirpated with the knife, and the disease returned after each operation, and ultimately destroyed the patient.† In this, as in other varieties of scrofula, although mercurial preparations are generally inadmissible, the proto-ioduret of mercury will be occasionally found useful. The cases adapted for this medicine are those in which the disease assumes the tubercular form.

Case 1.—Oct. 26, 1830. A lady, ætat. 52, consulted me on account of a painful, transverse contraction in the integuments, noticed by her about fourteen days, on the upper and outer part of the left thigh, below the trochanter major, which presented a puckered appearance, as if a string had been drawn as tightly as possible across the limb. The least motion of the extremity excited pain, and, in order to keep the thigh perfectly at rest, the patient sat with it in a constant state of extension. The disease resembled the angular scirrhous indurations which appear on the breasts of elderly females; an eruption of psoriasis in the ham, of the size of half-a-crown, and of the same character as the disease assumes in other scrofulous subjects, had existed two or three years.—Compound mercurial liniment, to be rubbed on the affected part every night.

Feb. 5, 1831. I had lost sight of my patient until this period, when she again consulted me. The hard, ligamentous-like bands now radiated in many directions, and became less prominent; absorption had evidently commenced; the indentation occasioned by the disease was observable in the integuments, and the pain continued. Yesterday evening a tumour, the size of an apple, was perceived in the extensor muscles of the thigh, and appeared to be connected with the keloide; it is sore and painful on pressure, and has an elastic feel, resembling that of fungus hæmatodes. The whole group has a very malignant appearance; a tumour, as large as a golden-pippin, distorting the thigh outwards, and a puckering of the integuments just above it, forming a deep and rigid fossa. The breasts are both healthy, and the menstrual discharge has ceased eleven years; countenance pale and emaciated.—Six leeches, and evaporating lotions to the tumour.

10. Tumour more flat, and less distinct; the diseased parts are now painful both when at rest and in motion.

24. Leeches and compression, with soap-plaster and a long roller.

March 3. The swelling more flat, and much softer. A small tumour now perceptible in the muscles adhering to the bone; this is painful, when the limb is allowed to hang down, as in standing or walking. The patient is obliged to be moved about in a Bath-chair.

31. Can stand and walk a little better.

April 14. Tumours unaltered in appearance; patient able to walk much better; the cord-like disease in the integuments, which had greatly subsided, has now increased.—Plasters and bandage discontinued; iodine liniment to be rubbed in every day.

From this time to the present, October, 1836, a gradual improvement has taken place, and walking exercise can be taken with ease, without any lameness being discoverable. The appearance of a cicatrix continues, and a small enlargement is perceptible in the deep-seated muscles. The pain has entirely subsided.

Case 2.—Nov. 19, 1833. H. W., a married woman, ætat. 25, advanced six months in pregnancy, had a disease in the lower lip, resembling the hard cicatrix following a lacerated wound, which extended from the coloured part of the lip to the cheek, in one direction, and in another, half way to the chin; she had also psoriasis on another part of the lip. The cancroide is very sore and painful. Duration of the disease about one month.

Iodine, gr. xxiv;
Linseed flour, ʒjss. Make 48 pills; two every second day.
Iodine, gr. xv.;
Suet, ʒss. Make a liniment, to be applied twice a day to the lip.

Dec. 3. Psoriasis removed; cancroide softer, and less elevated; the pain and soreness have subsided.

17. The scirrhus-like elevation rapidly diminishing.

Shortly after this period the remedies were discontinued, and when I saw the patient again, on July 29, 1834, all hardness and projection had disappeared, and the only vestige of the disease which remained was a depression and puckering in the adjoining integuments, occasioned by the ab-

† Surgical Observations on Tumours. By J. C. Warren, M.D., Boston: 1837.

sorption of the morbid deposit, and a white mark, like the cicatrix left by a wound occasioned by the bite of a dog.

In proof of the affinity of this disease to scrofula, I may observe, that on Oct. 29, 1835, this patient consulted me on account of a scrofulous ulcer on the elbow, and an abscess of the same character on the knee. At that time the mark left on the lip by the keloide was exactly the same as I found it in July, 1834.

Case 3.—Feb. 27, 1837. E. K., a girl, ætat. five, was admitted a patient of the Bridgnorth Infirmary. She had been afflicted during fourteen days with a disease in the skin of the abdomen, resembling the cicatrix left by an extensive burn. It was elevated above the adjoining parts, of a whitish-brown colour, of irregular figure, felt hard and dry, and was accompanied with much itching. Its circumference was well defined by a hard white ridge. The forearm, wrists, and hands were also affected and contracted with the disease.—To take daily four drops of the solution of *hydriodate of potass* with *iodine*, in cold water.*

Mar. 11. Porrigo favosa on the head.—Continue solution; *calomel*, 1 grain, every night. Ammonio-chloride of mercury ointment for the head.

20. Porrigo much better. Schirroid on the abdomen more flat and pale.

Several circular, flat, elevated tubercles, nearly white, a fourth of an inch in diameter, appear in front of the chest and abdomen, at a distance from the original disease. They resemble the schirrous tubercles found on the breasts of cancerous patients. The wrists and hands more flexible, and less indurated, and the skin covering those parts has assumed a pink colour, and affords some resemblance to lichen urticatus. Mouth a little sore; porrigo nearly well; several smaller eruptions on the abdomen. The original disease greatly relieved. Omit hydrarg. chlorid.

Nov. 14. The patient having been removed from the infirmary, I had no opportunity of seeing her till to-day, when I found the iodine had been discontinued and the disease had made rapid progress; both wrists and hands had been rendered helpless by the deposit of tubercular matter beneath the skin; two of the fingers were contracted immoveably on the palm; and one foot was swollen with a scrofulous abscess, occupying its entire upper portion, and extending towards the ankle. Seven or eight eruptions, resembling the cicatrices of burns, appeared on the chest, abdomen, and one shoulder, and the child was unable to rise from bed.

I recommended the practitioner in attendance to administer half a grain of hydriodate of potash twice a-day.

Dec. 4. The abscess has burst, and several decidedly scrofulous ulcers have followed; the small, white tubercles remain.

April 1, 1838. The child runs about, and seems well in health; the ulcers on the foot quite well; the white tubercles reduced in number and dimensions; the elevations on the skin have disappeared, and left immense, dark-brown marks, resembling cicatrices, on the shoulders, chest, and abdomen in front, and on the back and thighs. The child continues to take the hydriodate of potash.

Nov. 1. The tubercles have disappeared. The patches of discoloured skin now resemble marks occasioned by nitrate of silver just before they become black; the foot remains well; the elevations on the cuticle have vanished; a scrofulous ulcer, size of a crown-piece, on the wrist, which is carried in a sling. The iodine has been discontinued, contrary to my directions, several months.

The effect of iodine in this case has been very satisfactory. When its use has been continued, the disease has become mitigated; and when the medicine has been suspended, an aggravation of the symptoms has followed.

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* This preparation, which has been introduced by me into the pharmacopœia of the infirmary, is perfectly soluble in water, and produces the specific effects of the medicine with more certainty, and less inconvenience, than any other. Five minims contain one grain of iodine. The following is the formula:—

Iodine, 5 scruples;

Solution of potass, 1 ounce. Mix and agitate the fluid until the iodine is dissolved.

CONTRIBUTIONS

TO THE

PATHOLOGY OF CHILDREN.

BY

P. HENNIS GREEN, M.B.

NERVOUS TREMOR.

I do not remember to have seen this affection of the nervous system described in any English work on diseases of children, while, by several foreign writers, it has been confounded with chorea. There is, however, a striking difference between the two diseases. In chorea the movements are involuntary and distinguished by a great degree of irregularity. The patient finds it impossible to execute certain motions in a regular manner. In cases of "nervous tremor" the movements are also involuntary, but are far from exhibiting the characteristic irregularity of chorea. They are rather marked by oscillations occurring from above downwards, or from side to side.

The following cases will serve to give an idea of the progress and symptoms of "nervous tremor" as it occurs in children. With a little amplification, and some literary research, they might be made to constitute the foundation of a respectable "memoir," but they may, perhaps, be more useful in the present simple and unpretending form.

Case 1.—Eliz. Baulet, 13 years of age, a girl of weak constitution, and very small for her age, was admitted into the Children's Hospital on the 11th of February. On examining the child, the day after her admission, she was found labouring under a severe access of nervous tremor. The upper and lower extremities were quivering exactly after the manner of a person in the cold stage of ague. The child's intellect appeared to be obtuse, and it was impossible to obtain from her any information respecting her previous state of health, the duration of her complaint, &c. The general appearance of the patient was not indicative of the existence of organic disease; the skin was cool; the pulse 92; but on examining the chest a large cavity was discovered at the summit of the left lung. An opportunity now offered of questioning the child's relations, and it appeared that about three weeks previously she had been suddenly seized with the nervous tremor just described on learning the death of her mother. Since then the accesses were frequently repeated.

12. The fit of tremor continued for several hours, and then ceased; it was not followed by any febrile symptoms, or any appearance denoting the existence of intermittent fever. The patient continued in the same state for two or three days, after which the tremor became constant.

On the 20th February the little patient contracted the measles. The eruption was extremely irregular, affecting only a portion of the surface of the body; the stages of the disease were also irregular, but did not seem to influence, in any degree, the nervous affection. After the lapse of a few days the tremor was replaced by permanent rigidity of the upper and lower extremities. The head appeared as if fixed on the right shoulder; the arms were semi-flexed and immoveable; the lower extremities in a state of permanent extension. The patient complained of violent pains shooting along the limbs and down the vertebral column. The symptoms persisted, with little variation, until the death of the patient on the 14th of March.

The treatment pursued was merely palliative and insignificant, the medical attendant deeming it useless to fatigue the child with remedies of an active nature, as the cavity in the left lung rendered any chance of cure hopeless.

Necropsy twenty hours after Death.

Head.—The membranes of the brain were perfectly healthy. The dura mater, carefully examined, was found to be free from lesion; no trace of tumour or chronic inflammation. The arachnoid was perfectly transparent over the whole surface of the brain. The pia mater did not present any trace of albuminous infiltration, or other inflammatory product; there were no miliary tubercles apparent in any portion of the membrane. The lateral ventricles contained about a drachm of clear serum. The substance of the brain was not much injected; there was no trace of softening, tubercles, induration, or of any morbid change in any part of the hemispheres, or at the base of brain. The cerebellum and pons were equally healthy.

Spinal Marrow.—The membranes and substance of the spinal marrow were examined with particular care and minuteness. The membranes appeared to be perfectly healthy. The nervous matter was cut up into small portions, but no lesion or change of colour or consistency could be discovered.

Chest.—The superior portion of the left lung was the seat of a tuberculous excavation nearly the size of half a fist. The inferior lobe of the same lung contained some miliary tubercles, and was in a state of red hepatisation. The bronchial glands were converted into tubercular matter.

Abdomen.—The viscera of the abdomen were generally healthy. Some small tubercles were found in the spleen and underneath the peritoneal coats of the intestines.

Case 2.—Louise Germain, æt. 13 years, a strong, well-built girl, was received into the Children's Hospital, March 4. For the last two months the girl has experienced shooting pains along the lower extremities, which were immediately followed by accesses of trembling. During six weeks the attacks of pain and tremor were confined to the lower extremities, but for the last fifteen days the upper extremities, also, have been attacked in a similar manner. The patient has never experienced any headach or pain in the region of the spine. There have been no febrile symptoms. The appetite is good. The functions are performed with a sufficient degree of regularity. During the first few days which succeeded the patient's admission into the hospital no change in her condition was observed. The tremor of the limbs manifested itself from above downwards, and was always most marked when the child kept her arms in an elevated position. The pains along the extremities continue. The patient can flex and extend them without difficulty. The skin is perfectly cool; sensibility unchanged; pulse 92; mental faculties perfect; the senses of hearing and seeing unaltered. The patient was ordered to take a course of sulphur baths.

After the lapse of a fortnight the child endeavoured to get up and walk about the ward, but progression was still rendered extremely difficult, through the pains in the limbs and tremor. She found it much easier to ascend the stair-case than to descend it. The baths were continued.

The symptoms already enumerated persisted without any change until the end of March, when the patient began to complain of shooting pains, sometimes occupying the hypogastric region, at others the back or chest. A few leeches were now applied to the thighs, and the action of the baths assisted by a strengthening diet. This soon produced a beneficial effect. The menstrual discharge became established, and the girl soon left the hospital very considerably relieved.

Case 3.—Henri Guilbert, 13 years of age, apprentice to a house-painter, was admitted into the Children's Hospital, April 1. He had been seized, about four months before, with diarrhœa and symptoms of lead colic, which yielded to purgatives. On the 4th of January the child fell on his right side from a height of six feet, but was not seriously injured. Shortly afterwards he began to remark that the superior and inferior extremities of the right side were affected with involuntary trembling motions, and that his memory was feeble; he continued to work, but six weeks afterwards, having accidentally set fire to some wood-work, and being excessively frightened, he was again seized with vomiting, colicky pains in the abdomen, and tremor of the arms and legs.

April 2. The patient complains of violent pain in the head: the face is flushed; tongue clean and moist; little thirst; no difficulty of deglutition; no vomiting or nausea; abdomen retracted and painful to the touch, especially in the neighbourhood of the umbilicus. When the anterior wall of the abdomen is pressed from before backwards it gives pain; but, on the contrary, relief, when pressed from below upwards; no evacuation for the last three days; some dry cough; sibilant râle; no dulness of the chest on percussion; skin natural; pulse 92; respiration 28; the arms and legs are affected with nervous tremor, which is but little apparent while the child lies down, but becomes extremely marked as soon as he endeavours to sustain a limb in a particular posture. The motions in this instance are regular, and executed from below upwards; the general sensibility of the skin is unchanged; the boy can squeeze an object between the fingers with undiminished force; there are no pains in the neck or spine. When the boy walks about in the ward, the body appears to incline to the left side, and the right leg seems to drag a little.—Bleeding to 8oz.; mustard foot-bath; milk diet. A quarter of an hour after the bleeding the patient vomited two or three times.

3. The nervous tremor continues undiminished, and seems to have extended to the tongue. The patient complains of headach, confined to the right side of the head; he also experiences some colicky pains about the umbilicus and epigastrium; the vomiting has returned once or twice; skin cool; pulse natural.—Twelve leeches to the painful part of the abdomen; purgative enema.

4. At four o'clock in the afternoon the patient was seized with contracture of the limbs on the left side of the body; the jaws became fixed; the mouth foamed, while the right arm and leg lay in a relaxed position; the fit continued for five or six minutes, during which the boy lost all consciousness. In the night he became agitated, and then so violently delirious as to require to be tied down in bed.

5. The patient is much depressed; the face is pale; he complains of pain in the forehead, but answers clearly all questions asked.—Six leeches to the temples; sinapisms to the legs.

6. The child has slept well during the night; no delirium or agitation; the tremor continues, as also do the pains in the abdomen and loins; he still complains of headach.

7. Spontaneous discharge of a quantity of fluid matter from the bowels; this was soon followed by a remarkable change for the better; the face assumed a natural expression, the headach ceased, and the tremor of the limbs was scarcely perceptible.

8. The diarrhœa has ceased and the bowels are confined. This state was allowed to continue for a few days.

On the 12th, a purgative enema was ordered, and continued daily. On the 16th, the child was placed in a bath, but was immediately seized with delirium; he was taken out, and a purgative enema administered without delay. The tremor had now almost completely disappeared.

On the 27th, an ounce of castor oil was given; this produced abundant evacuations and dissipated the colicky pains, which had never ceased to torment the patient. This mode of treatment was now followed up. The patient took active purgatives by the mouth, and was discharged, completely cured, on the 10th of May.

Remarks.—Nervous tremor is defined, by Joseph Frank to be "a rapid and equable oscillation of the limbs, which are agitated to and fro with a constant motion." That form of it which is produced by opium or tobacco, or by exposure to the vapours of lead and mercury, is well known to writers on diseases of the nervous system, but to the other forms little attention has been paid, as any one may convince himself, who will refer to the article "Tremor," in Good's "Study of Medicine," or the more recent observations of Dr. Marshall Hall,

on Tremor Mercurialis and Paralysis Agitans."† That the affection should be arranged under the class Neuroses, is proved by the cases just related. In the first case, which terminated fatally, the whole of the central nervous system was examined with the greatest care, but no lesion could be discovered. The disease was probably occasioned by mental emotion, but the presence of contracture of the limbs, with shooting pains, and pain along the spine, was calculated to mislead the judgment, especially as the child suffered under pulmonary consumption. Indeed, nothing prevented the diagnosis of "tubercle of the spinal marrow" from being given, except the fact that this lesion of the nervous system is extremely rare, even in children. The case is an instructive one, and shows how very circumspect the medical man should be in the diagnosis of organic lesions of the nervous centres. In case 2, the nervous tremor was evidently occasioned by the derangement of the system previous to the establishment of the catamenial discharge; while, in case 3, the exciting causes of the malady were complex, the most manifest being the influence of lead. The treatment of this latter case was excessively timid and defective; had the boy been freely purged on his admission into the hospital, it is evident that all the symptoms of the complaint would have been speedily dissipated. Amongst the causes of tremor enumerated by Frank, are mental emotions, fright, and difficult menstruation; he also regards atony of the system and cerebral congestion, as frequent exciting causes. The latter, however, more commonly refer to adult persons, for atony or debility are more apt to induce chorea in the child.

ENCYSTED TUMOURS
SUCCESSFULLY TREATED BY
PUNCTURE AND INJECTION.

To the Editor of THE LANCET.

SIR:—Not being aware that the above method of removing encysted tumours has been previously practised, and being prevented by the appointment that I now hold from gaining, personally, further experience as to its efficacy (no person having such deformities being admitted into the naval service), I am anxious, through the medium of your Journal, to ascertain whether the same operation has been practised by any one else, and, if so, to be informed of the results of it. In October, 1833, I was first induced to try it in the case of a young woman, residing in one of the southern counties of Scotland, who, from childhood, had been disfigured by a tumour of this description, situated on the right temple, which, in size and shape, resembled an egg. As usual, its growth had been very gradual, and unattended with pain, being, when first observed, about the size of a pea, situated at the external commissure of the eyelid, whence it had extended, outwards, over the temporal aponeurosis, being anteriorly covered by the concentric fibres of the *orbicularis palpebrarum* muscle, as was very obvious from the effect produced on the tumour when the patient attempted to close the eye. A few years previously my friend Mr. Manners, surgeon in Berwick-on-Tweed, had removed from my own person a tumour precisely similar, when, in order to reach the cyst, he found it necessary to divide freely the fibres of this muscle, which, by the gradual increase of the subjacent tumour, were spread out in a layer between the integuments and cyst. For a very long time afterwards I had the greatest difficulty in voluntarily closing the eye of the affected side, and, at the present hour, the opening and closing of both eyes can only be performed synchronously by a considerable effort. From the similarity that existed between the two tumours I hesitated to perform an operation that was likely to be attended with scarcely less inconvenience than the tumour itself. I resolved, instead, to puncture the sac with a lancet at its posterior portion, where it was not covered by the fibres of this muscle, to express its contents, and then inject some stimulating fluid, with the intention, however, of having recourse to the usual practice in event of its proving unsuccessful, or any unpleasant consequences ensuing. The puncture being made, the *atheromatous* contents of the cyst were evacuated by the spasmodic action of the muscle alone, which continued for upwards of a minute; the sac was then injected with coarse "grain whiskey," the stimulant most accessible in a northern cottage. This was retained for about a minute, during which the patient complained of a slight burning sensation. A compress, moistened with vinegar, was then applied over the site of the tumour, and secured there by a bandage, the eye being kept covered up. On removing the dressing three days afterwards, not the slightest vestige of a tumour having existed was perceptible, except the red spot that indicated the site of the puncture. Between that year and August, 1836, equally successful results followed the same operation in five other cases; in two the tumours were somewhat larger than a walnut, and situated on the scalp, the patients being a man and woman, both above fifty; in one case, that of a young man about twenty years of age, the tumour extended from near the centre of the forehead down to the very margin of the right eyelid; in another, a young married woman, it was situated on the left side of the nape of the

† LANCET, vol. ii., 1837-8, p. 41.

neck, and was about the size of an egg; and, in the last, that of an old pedlar, it was of a pyramidal form, occupying the angle between the nose and cheek, and had attained such a size as materially to obstruct the vision of the corresponding eye. In three of the cases the contents consisted of *brain-like* matter; in two, of a *glairy fluid;* and in one, they resembled most closely the *yolk of a hard boiled egg* in appearance, though not in consistence. The fluid injected in one or other of these cases was either common grain whiskey, brandy, or a solution of common table salt in water.

With the exception of the pedlar, I had an opportunity of seeing all the parties operated on in July last, and in no instance had the slightest tendency to a recurrence of the tumour evinced itself; indeed, in all of them, after the first week, the redundant integuments had so contracted that no one could have suspected them to have formed so recently the walls of such bulky protuberances. The extreme simplicity of this operation has much to recommend it; not only is it attended with less pain, but with little or no disfigurement, which last advantage will entitle it to a decided preference over the operation by excision, should farther experience demonstrate its results to be equally satisfactory in the practice of others as in mine. The situation which these tumours generally occupy being the head, neck, and face, the scar left by the common operation has been the means of blighting the prospects of many an otherwise agreeably-featured female, and even of our own ruder sex being blemished with such unseemly scars, a stranger is apt to ascribe a pugnacity of character which we may not deserve. I have the honour to be, Sir, your very obedient servant,

ALEX. A. CARR,

Member of the Royal College of Surgeons, Edinburgh, and Assist.-Surg., R.N.

Sheerness Dockyard,
Dec. 11, 1838.

P.S. I would not for a moment have it supposed that I consider this mode of treatment applicable to the very large encysted tumours that are occasionally met with, but only to those which we fall in with daily on the head, face, and neck, not much exceeding the size of an egg. I may remark, however, that I am convinced that in the operation by excision the dangerous consequences of allowing any portion of the cyst to remain behind has been greatly over-rated. I have sometimes failed in attempting to extirpate it completely, in which cases one of the above stimulants was applied to the raw surface; in no instance did any bad results follow.

SALTS OF MORPHINE. ADMINISTRATION OF MERCURY. CONSTIPATION.

To the Editor of THE LANCET.

SIR:—I am sorry to see in your last week's LANCET a statement, taken from M. Raspail, regarding morphine and its acetate, unguarded by any note or remark of your own. Should any beginner read it, who is about to use morphine for the first time, the chances are that a dangerous dose may be administered. I have used morphine (acetate and muriate) often, but in severe neuralgic cases, or in suppuration round nerves which, more than mere neuralgy, produces agonising pain. I have not been able to use more than two grains at a dose without producing lengthened and excessive stupidity, and this not more than thrice repeated in twenty-four hours. Venturing further, I have been warned by incipient subsultus tendinum to desist. I have known a grain (I speak of the acetate) produce, in an adult, a long and doubtful coma; doubtful, I mean, during its action, as to the result. I have known half a grain, in partial suppressio urinæ, completely to stop the secretion, and, thus, doubly, that is, directly by its narcotic effect, and indirectly by the suppression, to produce coma. My experience, extensive, though short in time, teaches me never to begin with more than one-third of a grain of the genuine drug; gradually I go on to the quantity deemed requisite, and thus I have given to an infant, three months old, as much as one grain in twenty-four hours, having previously crept on very cautiously, taking care that the *secretions are freely going on.* I suspect, from M. Raspail's recent work on botany, that he is, even in scientific subjects, very much of a visionary; in this it appears that he is mischievously so.

While I am writing I will advert to two or three other matters.

I think, while there are so many purgatives of more innocent qualities, it can scarcely be right, in obstinate and long-continued costiveness, to administer much mercury. Especially should it not be repeated day after day, the bowels being inactive all the time. In simple costiveness, a pill of mercury, at night, followed by a brisk saline in the morning, just before a warm tea or coffee breakfast, may be very good, but if no action be produced I should give no more mercury. In a fit of constipation, having existed for some length of time, I should scarcely venture to give a dose of mercury, especially if the patient have ever been freely salivated before,—a process that renders resalivation often more easy than desirable. Two ladies whom I attend, warn me of the mercurial which I may have ad-

ministered, although it has been only half a grain of blue-pill, and solely owing to previous free salivation do they thus readily detect the mercurial, a little springing pain and heat about the gums being the earliest sign complained of. Mercury after scarlet fever, or given during its course, is, as is commonly known, excessively destructive; so it is after measles, or in any fever, or even, generally, in disease of low vital manifestation. Two cases of cancrum oris, in one of which the destruction of the jaw, cheek, and gums, was frightful, taught me never to forget the danger of even the smallest doses of mercury in diseases of low vital power, especially in children. I will not say that if mercury be specially required, even in these cases, the danger is so great as to prevent its use. I wish only to say that in such cases constant watchfulness and care are required, and dose must not be given upon dose.

All this is probably known; still, Mr. Editor, there is no harm in exciting attention to it, especially as some few remarks have been recently made in your Journal respecting them.

Constipation during and previous to Parturition. — The great evils of this state of things have been often forced on my attention, and, although obvious to all, yet these remarks may not be thrown away. Recently a case of mine, of this sort, somewhat astonished me. During the parturient efforts I observed, on seeing the tongue, a red streak in the centre; consequently I inquired about the state of the bowels, and was assured that they were in correct order. Subsequently five or six quarts of black putrid matter were evacuated, and gastro-enteritic fever set up. In other cases, when the rectum contains much hardened fæces the contraction of the sphincter, voluntarily, from delicacy, is sufficient, for a long time, to prevent the expulsion of the child,—the small diameter of the anus requiring comparatively little force to enable it to counterbalance the uterus and abdominal muscles; and this, in a protracted first labour, from age, may become a very serious impediment. True, an injection may be used during labour, but it is a bad plan, especially when it is in our power to make an it axiom of British midwifery that, prior to labour, as much so as prior to lithotomy, the bowels be emptied by two or three injections of warm water, &c.,—not by purgatives by the mouth; these often produce the effect which you desire least, just bringing the fæces down to the rectum, where a little previously-collected hardened fæces stop them, and all agglomerate together. I could enlarge much on this, but I am sure I have for once written enough. I am, Sir, your obedient servant,

W. RENDLE.

Dec. 12, 1838.

STATISTICS OF LUNATIC ASYLUMS.

To the Editor of THE LANCET.

SIR:—As the proprietor of a private asylum for the reception of insane persons, I feel called upon, in self-defence, to publish a statistical account of the results of the treatment of the insane in my establishment in the columns of THE LANCET, together with some accompanying remarks, as soon as your convenience will allow of their insertion. The number of patients is comparatively small, but who can wonder at that, who considers the efforts which have been made, and are being made, to destroy the character of the private establishments, in order to bolster up the county asylums, and large joint-stock concerns, &c.? Now, I believe, if strict investigation is made, it will be found that in private establishments, under the proprietorship and superintendance of medical men (not those conducted by non-professional persons), the greater number of cures are effected on an average, and more satisfaction given to the patients themselves and their friends. I might enter largely into this question, and state many valid reasons why it should be so, and also some rather astounding facts, which, in the course of my experience, I have learned, as to the treatment of lunatics in some county asylums, and I should, by doing so, come to conclusions exactly opposed to those put forth by the patrons or proprietors of the "Invalid Institution;" but this is not the time, or place, to enter further into that subject; I will merely add, that unless they can succeed in interweaving into their system some of the principles and motives which operate in the management of a well-conducted private establishment, they will find, after all, when notes come to be compared, that they must fall back, and give to the latter the first place in the public estimation. I do not believe that any public institution of the kind, on a large scale, with salaried officers, can supply the permanent zeal, the diligence, and watchfulness, and the anxiety to promote the cure of the patients, and the successful conduct of the establishment, which is to be found in private asylums, conducted by respectable professional proprietors, on whom rest the whole of the responsibility. In proportion as this responsibility is divided, as in large institutions, it is weakened, and the constant tendency of the management is to become first indifferent, and lastly apathetic.

I shall, with your leave, publish the results of my past exertions in the cure of lunacy, and if I have opportunity, and the private establishments are not annihilated by the attacks now being made upon them, openly and covertly by parties not always, I fear, the most disinterested, the results of my

future efforts. I could wish that other professional proprietors would do the same; we should then be able, shortly, to come to a satisfactory conclusion as to the best system of curing lunacy. The following table shows the result of the treatment of the patients received into the Driffold Asylum, from July 1836, to November, 1838, inclusive:—

	Males.	Females.	Total.	Cured.	Remain under Treatment.	Died.	Removed not Cured.
Monomania .	7	4	11	6	3 { 2 curable / 1 doubtful }	..	2
Mania.....	4	1	5	4	1 incurable	—	—
Dementia ..	1	1	2	....	.....	2	—
	12	6	18	10	4	2	2

Of the two monomaniacs removed, one remained under treatment five weeks, but his disorder being complicated with consumption, and the inevitable event being foreseen, he was removed home, where he died in a few weeks of the latter disorder; the other was removed on the score of economy. The two cases of dementia were paralytic when brought in; the one died of apoplexy, the other of the effects of a disease of the heart of long standing. There then remain fourteen, of whom ten are cured; two will probably be cured; one is doubtful of cure, and only one, who has been deranged many years, appears to be incurable; thus, of fourteen patients admitting of a possibility of cure, one only would seem to present insuperable difficulties. In my last letter I explained some of the means I have employed for the purpose of restoring a sound condition of some of the bodily functions; there are other points regarding this important subject which I cannot enter into now; I believe a judicious method of medical treatment in lunacy is far from prevalent.

In conclusion, I beg leave to add, that in discharging my patients, cured to the best of my powers, and at the same time my conscience of all accusation in this respect, gratifying as this is to my feelings, still I have to regret the apathy and inattention of the medical profession. How is it, Sir, that patients are crowded together within the limits of stone walls, in the precincts of London, and other large towns, whilst the healthiest spots in the country, with thousands of acres to roam over, in pursuit of game, or other amusements, affording novelties almost daily, with the varying and untiring scenes of farming operations, are neglected? Perhaps you can explain this; in the mean time, from the result of the table above given, which shows but four inmates remaining, I am obliged to direct public attention to the fact, in order to supply the vacancies of the patients discharged cured from this asylum. I firmly believe that no one superintendant can properly undertake the charge of more than twelve patients, and do them justice, and to that number I limit my asylum. I remain, Sir, your obedient servant,

GEO. BODINGTON.

Driffold Asylum, Sutton Coldfield, Warwickshire, Nov. 28, 1838.

DECOMPOSITION OF THE FŒTUS IN UTERO.

To the Editor of THE LANCET.

SIR:—I beg to transmit you, for insertion in THE LANCET, the accompanying singular history of a patient who has been under my observation:—

H. Middleton, about 27 years of age, and the mother of four children, was in her fourth month of pregnancy in July last, when she received a severe kick upon the pubes. During the first fortnight after this accident she experienced almost continued pain in the region of the womb. At the end of that time the waters were discharged, followed by profuse hæmorrhage, which, however, always ceased on getting out of bed. The hæmorrhage continued, with intermission, for ten or fourteen days, alternating with copious vomitings of blood. At this period of the case the rib-bone of a fœtus was discharged. On examination, the whole of the remaining bones of the fœtus, separate, distinct, and free from any soft parts, were removed from within the vagina; the bones I have by me, and they constitute a complete fœtal skeleton. The discharge from the vagina was not perceptibly fœtid; the placenta followed the bones, firm and healthy in structure, and apparently but just separated from the parietes of the womb. The woman has completely recovered. I am Sir, your obedient servant,

T. E. RAWSON.

Kegworth, Dec. 7, 1838.

ENORMOUS WEIGHT OF A CHILD AT BIRTH.

To the Editor of THE LANCET.

SIR :—I beg to forward you the following case which occurred in the course of my attendance on the patients of a public lying-in charity. I believe it refers to the largest fœtus, perhaps, without exception, on record. Dr. Conquest gives the average weight of the fœtus at seven pounds. The one which is described in the accompanying case weighed more than seventeen pounds. Yours, most obediently,

J. D. OWENS.

Haymoor, near Ludlow,
Nov. 12, 1838.

Mrs. R., a poor woman, residing in Pulteney-court, Little Windmill-street, was taken in labour at 8 o'clock, A.M., Nov. 6, 1838. She was a person of middle stature, square set, and of good health. The present made her sixth accouchment. Her first child was still-born, after a lingering labour. In her fifth labour she brought forth twins, one of which was born dead. None of her children lived longer than sixteen months, and all of them were remarkable for size and length. She stated that she had experienced periodical labour-pains every evening for the past fortnight. She supposed that she had gone beyond the usual period of utero-gestation ; the catamenia had ceased to appear in the first fortnight of January last, being forty-two weeks since the absence of that secretion. We found her in the erect posture, supporting herself by the bedstead. The membranes had been ruptured about an hour, and the liquor amnii had been discharged in considerable quantity ; it was of a greenish-yellow colour, apparently from the admixture of meconium. Uterine action was slow and feeble, with intervals of three minutes. Having satisfied ourselves that the head presented, we went away, requesting to be sent for directly the pains became more decided.

At 4, P.M., we were again sent for; the uterus was now in full action. The usual examination detected the head at the outlet of the pelvis, but its long diameter was not exactly in correspondence with the long diameter of the outlet : it was placed somewhat obliquely, having the right ear corresponding to a point midway between the symphysis of the pubis and the tuber ischii of the left side, the sagittal suture, of course, not being in the median line of the sacrum. As each successive pain forced the scalp on the perineum we made every exertion to correct the presentation, trusting the energy of the uterine system yet remaining would expel it, although we were aware of the large-sized head of the fœtus, and its highly ossified condition ; but the hand alone could not effect it, for the sense of touch was soon benumbed. This state of things having existed for five hours we requested the assistance of Mr. William Bloxam, lecturer on midwifery, who, by means of the vectis, brought the head of the child into the proper position, and a few pains more accomplished the delivery. The correction was readily effected by introducing the vectis over the head of the child, behind the left ear, and placing the index-finger of the left hand on the right side of the forehead ; then, by depressing the occiput with the instrument, and at the same time elevating the forehead, the head was caused to rotate in such manner as to throw the forehead into the hollow of the sacrum, and place the occiput exactly beneath the symphysis pubis. It was thus reduced to a natural presentation. The crown of the head now protruded through the os externum, but uterine action was so much exhausted that we had to wait an hour ere the head was expelled. The width of the shoulders now presented a new obstacle ; we therefore, after some difficulty, brought down an arm, but forty minutes more elapsed before the child was disengaged from the mother. During this tedious and trying occasion stimulants were given freely. The child exhibited no signs of life, and appeared to have been dead some hours. The funis was dark and livid, and it was coiled over the right shoulder, again round the body, and passed away over the right ankle. The perineum escaped uninjured. The placenta, after considerable delay, and we had employed every means to cause the uterus to contract, was expelled by the natural efforts. It exhibited the same hypertrophied condition (if we may use the expression) as was remarked in the overgrown child. Hæmorrhage, which threatened to be severe, yielded to the usual means. The woman was now composed by an opiate draught, after a labour of sixteen hours duration, and is now doing remarkably well. A few piles, and some difficulty in making water, were noticed on the second day, which admitted palliation by simple treatment.

We add the weight and admeasurement of the child, as taken ten hours after delivery :—The long diameter, from the occiput to the root of the nose, 7½ inches ; the occipito-mental, 8½ inches; from the parietal protuberances, 5 inches; the circumference of the skull, 15½ inches; the circumference of the thorax over the xyphoid cartilage, 14½ inches; the breadth of the shoulders, 7½ inches; the extreme length, 24 inches. The weight, seventeen pounds, twelve ounces.

LETTER FROM AN ENGLISH SURGEON AT AUSTRALIA.

MANY inquiries from English surgeons who have been anxious to emigrate to South Australia, induce us to believe that the following short extract from a letter will afford interesting information to a large number of medical men, though we give it insertion with regret that intelligence from so distant a quarter should be invitingly welcome to any highly-educated, intelligent, and useful, though ill-remunerated members of the community at home. The letter is addressed to his relations, by Mr. W. WYATT, formerly a surgeon in Plymouth. It contains no information of a professional character, the topics being limited chiefly to such as relate to the means of subsistence in the new country, and, from that reason, might as appositely have come from a settler of any other previous calling or profession; but the production of an emigrant who may have quitted England under many of the same circumstances, and with the same expectations, as those among his professional brethren who desire to find a land flowing with more milk and honey than is to be found in the free places of Great Britain, will be more acceptable in the pages of a medical journal than the letter of a tradesman or a farmer. The absence of all reference to professional pursuits or prospects, however, may be accounted for from the new duties of Mr. Wyatt being partly magisterial and official, and partly from the fact, that he has not found any *patients* in Australia, either to claim his own attention or that of any townsman who may be qualified to practise medicine. This circumstance is consistent with the remark of a correspondent who, in his caution to medical emigrants, in THE LANCET of Feb. 10, 1838, thus wrote:—"The Australians have neither endemics nor epidemics, and the consequence of this and other favourable peculiarities is, that they enjoy *health of the highest order*." The letter of Mr. Wyatt is dated April 2nd, 1838, at which time he had been rather more than twelve months at Adelaide, the settlement in South Australia whence it is written. The circumstance relating to the vicissitude of temperature in the same day, noticed by Mr. Wyatt in the last paragraph, is briefly but ably discussed in a paper by Mr. Dempster in THE LANCET of May 20, 1837, when explaining the advantages of the climate of Australia to invalids leaving India for a change of air, temperature, and scene:—

"In many respects our colony is fast advancing; stock of all kinds is daily increasing, and provisions, articles of clothing, iron ware, and other goods for domestic purposes, are in sufficient quantities, though still very dear. I cannot too often repeat that a general cargo from England would be sure to pay any one handsomely, and yet be under the colonial prices. Whoever thinks of coming out should be particularly careful to provide themselves with the most useful fittings of a house, such as locks, bolts, hinges, window-fastenings, and the whole host of et ceteras, which may be seen by looking over their own houses. In fact, they can scarcely make a mistake, for few things can be had here as cheap as in England. We have now stores of all descriptions, and there is a very considerable influx of settlers from the neighbouring colonies, especially Tasmania. Indeed it is certainly a most excellent opportunity for those people to better themselves, seeing that they are at so trifling a distance, that they may come supplied with every necessary, and almost every luxury, at a much lower rate than the original colonists. So also, persons now arriving from England, do so more advantageously than we did, who have had to bear all the hardships and extra expenses of a new establishment. It is true that we have, in a great degree, been compensated by the purchase of town lands which have increased in value at least five hundred per cent. since the sale; that, however, may vary considerably, according to circumstances, and I think will fall, at least for a time, when the country land comes to be selected and occupied.

"Some persons have made a good deal of money by gardens, and the colonial gardener, Allen, has grown between one and two thousand melons, which he has been selling from 2s. 6d. to 6s. a piece, cabbages have made 6d. each (one weighed 13lbs. after the outer leaves were taken off); green peas 1s. 6d. per pint; potatoes from 4d. to 6d. per lb.; cucumbers from 6d. to 2s. 6d. each; lettuces, spinach, parsley, &c., in proportion. Next summer we shall no doubt have an abundant supply of vegetables of all descriptions, and probably very cheap; and as there are many cattle, sheep, and pigs, arriving in every ship, and a great number expected overland from Sydney, and other places, we may look for a fall in the price of meat.

"The friends to the industrious poor in England can do no better for them than to send them to South Australia. Should any from Plymouth be disposed to come out, you should encourage them by all means; and it will be a great pleasure to me to do anything in my power for any who may come recommended by persons of responsibility. If I had the time, I think I should be tempted to write to the merchants of Plymouth whom I know, respecting the advantages which would attend a regular system of trading to South Australia, but I hope they will find this out before long without my assistance.

"I should like to see some of the benefits which must arise out of this new province of the British Empire enjoyed by my fellow-townsmen; and it would be no small gratification to me to be one means of associating in the bonds of commerce my own dearly loved native town, and my adopted country. It is by no means a romantic opinion that this almost antipodal region will one day become another British Empire: at any rate Nature appears to have done her part toward it. Our province alone, which is but a trifling portion of this extraordinary country, contains more than 200,000,000 of acres, with a climate well adapted to European constitutions.

"With regard to the climate, although variable, I think it healthy, and it is really astonishing how well we bear the sudden changes of temperature, which in England would be productive of very bad consequences. A fall in the thermometer of twenty degrees is thought nothing of, and people scarcely put on an extra garment for such a change, which often occurs in a few hours. Indeed, under all temperatures, we see persons indifferently clothed in all varieties of dress, from a warm English broad suit, to the light white jacket and trowsers of the warmest latitudes. In no article is this variety more amusing than in the hat. Such a multiplicity of forms, hues, and textures would no doubt make a new comer fancy himself at a fancy dress ball, but the initiated see these things until they have forgotten that there ever was such a thing as fashion, or anything at all approaching to uniformity in dress any more than in features."

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, December 11th, 1838.

Dr. BRIGHT, President.

FRIGHTFUL SPREAD OF SMALL-POX THROUGHOUT LONDON AND THE PROVINCES; ITS INCREASE ON THE CONTINENT.—VACCINATION ONLY A TEMPORARY SECURITY.—INFLUENCE OF RE-VACCINATION.—NATURAL AND MODIFIED SMALL-POX.—MORTALITY OF SMALL-POX AFTER VACCINATION.

As soon as the minutes of the last meeting had been read

Dr. MERRIMAN rose, and said, that he wished to call the attention of the Society to a subject of very great importance; he alluded to the great increase of small-pox in the metropolis for some time past. He was induced to address the Society on this occasion, in consequence of a circular which had been issued from the Small-pox Hospital, calling on benevolent persons to subscribe towards a fund for the purpose of increasing the accommodation of that institution. It appeared, from the Report published respecting the Small-pox Hospital, that out of every five cases of small-pox admitted, three were altogether unprotected, either by inoculation or vaccination; the other two had been vaccinated. Now, he had not the slightest doubt of the correctness of this return, though he hoped the fact did not hold good with respect to small-pox generally. If it did, however, one of two things must have occurred: either vaccination was much less effective as a preventive of small-pox than generally thought, or else it was performed in an improper and imperfect manner. He had known a few instances in which several children of a family were revaccinated; in some the vaccine vesicle proceeded as though the patients had not before been operated on, while in others the result was a mere pimple, or angry pustule, which entirely disappeared in a few days. The statements from the Small-pox Hospital were frightful, and he thought the question which he had opened was one well deserving the serious attention of the Society.

Dr. GREGORY said that the facts brought forward by the last speaker were strictly correct, as far as he (Dr. G.) had been able to make them so, having drawn up the statement himself. It would appear, that in the middle of November of last year there was a sudden and remarkable increase of the number of the patients admitted into the Small-pox Hospital; this increase continued during the months of December, January, and February. He was unable to account for this, but thought it probable that the disease was unusually epidemic in London, and would soon subside. This, however, proved not to be the case; the numbers had continued to increase up to the present time, patients being received from all parts of London; to such an extent had small-pox increased, that should it go on in the same ratio the hospital must be by some means enabled to give more accommodation than it could do at present. The largest number of cases admitted into the hospital, in any one year, since its foundation in 1746, was in 1781, a year in which small-pox was remarkably prevalent and fatal. The number admitted in that year amounted to 646. In consequence of this vast increase the Small-pox Hospital, which was then situated in Cold-Bath-Fields, was unable to receive them, and the building now known as the Fever Hospital was erected to accommodate the additional patients. In 1838, from the 1st of January to the present day, the enormous number of 681 had been received into the Small-pox Hospital, being an increase of 35 on the year 1781. What rendered this statement more extraordinary was the fact, that in 1781 all the patients were unprotected either by inoculation or vaccination. Of the patients received this year, 281 had been vaccinated. He had collected information from various sources

to ascertain if the small-pox had spread to the same extent in other parts of the world, and also with respect to vaccination and its influence. The disease was spreading in the same fearful manner throughout England, scarcely a town or village having being free from it since last November. He had been furnished with this fact from the Committee of the Provincial Medical Association. With regard to the question of the numbers vaccinated and unvaccinated who had suffered from small-pox, he had not yet received an answer. Information fromthe Continent was less accurate, but the increase throughout Europe had been great. In 1829, the principal Governments of Germany took alarm at the rapid increase of small-pox, and resorted to revaccination as a means of checking it. In Prussia, 300,000 had been revaccinated, and the same number in Wurtemburg. In Berlin nearly all the inhabitants had undergone the process. In Paris the same plan was pursued; and in Denmark, nearly the entire army had been re-vaccinated. In the years 1833 and 1834, small-pox prevailed to a great extent in Copenhagen; in 21 months 1000 patients were admitted into the hospital with the disease; of these 900 had been vaccinated and 100 not so. It might, therefore, be considered, that in Denmark the numbers vaccinated were as ninety in one hundred, and taking the mode of calculation from the numbers admitted into the Small-pox Hospital in this country, 40 were vaccinated, 60 unvaccinated, who received small-pox. How were we to account for this great difference between the persons affected in England and Denmark? In Denmark, and certain parts of Germany, vaccination was rendered imperative by the Government; and not more than ten per cent. of the population were found "contumacious," or refusing to be vaccinated. Now, the patients admitted into the Small-pox Hospital within the last few months had made the numbers increase to nearly 50 vaccinated, and 60 unvaccinated. On the whole therefore, he had been induced to draw the conclusion, that in England, where vaccination was not rendered imperative, the proportion of vaccinated persons affected with small-pox, was as 50 to 70 unvaccinated.

Mr. DAVIS was convinced that one of the causes why vaccination failed to be a protection from small-pox, consisted in the fact, that numbers of persons, after their children had been vaccinated, never returned to the operator to ascertain if the proceeding had been successful. For many years he had carefully registered a great number of cases, and of such as he had been able to watch, not one had failed in affording the individual protection, though small-pox of a most malignant character frequently prevailed in the neighbourhood where they lived. Some years since, a travelling woman brought small-pox into a village in Hampshire; the disease was malignant; an infant was at her breast, and drew nourishment from the mother till she died. The child was vaccinated, and did not take the disease, nor had suffered from it at the expiration of several years.

Dr. YELLOLY had no doubt that very severe small-pox occurred after vaccination had been completely performed in the affected persons; cases of this kind had happened in his own family. Some of his children had been vaccinated by Dr. Jenner; the others by a most experienced and careful vaccinator. Small-pox existed in his (Dr. Y's.) neighbourhood; five of his children contracted the disease, two of them had it in a mild form; in the others, it was most severe, and in his eldest son the eruption did not go off until the tenth day. He might mention one fact in regard to vaccination:—A boy, who had been vaccinated, was admitted into the Norwich Hospital with modified small-pox, and recovered. A porter in the hospital, who had suffered from natural small-pox, took it a second time from the boy, and died.

Mr. POWELL inquired whether the great proportion of cases of small-pox, after vaccination, were not modified.

Mr. ARNOTT thought it would be satisfactory also to know, at what time after vaccination, small-pox usually appeared; and what was the rate of mortality, and the severity of the disease, in the affected persons. He asked this question now, because the public mind was at present much agitated respecting the subject of vaccination and its influence on small-pox. Facts could no longer be disguised; the subject must be investigated.

Dr. GREGORY replied, that observation was much in favour of a general modification of small-pox as occurring after vaccination. He was now preparing returns on this point from 600 or 700 cases, and these would show a modification of 60 per cent. in vaccinated persons. Of the 40 in whom the disease was unmodified, the result was the same as though the individuals had not been previously vaccinated; the mortality would be the same. In 100 cases of small-pox occurring after vaccination, the deaths would be nine; in 100 cases, in which the persons were not protected by vaccination, the mortality would be about 25. He had certainly known it some years to be as low as 17 per cent.; but 25 per cent. would be found to be an average. Now, it had been supposed that in the 40 persons in whom unmodified small-pox occurred after vaccination, the vaccination had been, in point of fact, merely nominal, the ceremony only having been gone through, and the patient consequently not been protected. Investigations, however, had proved that this statement was not to be relied upon; and he was prepared to say, from most careful de-

ductions, that the idea of imperfect vaccination in the forty persons would really hold good with respect to a very few indeed of them. He now came to the most important question concerning vaccination, and that was, the influence which time exerted over its preventive power. He had no sort of doubt now, that vaccination, when perfectly performed, would protect the child for a certain time. It was no proof that the vaccination had been perfect because the child had been exposed to small-pox and did not receive it. Puberty, and other changes which took place in the system, between the ages of 15 and 25, put vaccination to the test. All the severe cases of small-pox after vaccination, which he had seen, had, without a single exception, occurred fifteen years after the latter affection. He had seen no severe case which occurred at a shorter period than this. One or two very mild varioloid cases had occurred in children between eight and nine years of age; none, even of this form, earlier. The earliest death from small-pox after vaccination was at fifteen, and the mortality increased as the persons advanced in life, the greatest number occurring from 25 to 30. He would go one step further than this: the character of the cicatrix excited great attention on the Continent, and to such an extent were ideas founded on this character carried in some parts, that a theory had been established to the effect, that in proportion as the cicatrix was more or less circular and well defined, in such proportion would be the character of the disorder in after life. But the character of the cicatrix gave no evidence of the success of the vaccination; the cicatrix was also no criterion of the character of the small-pox which might occur. In the very worst cases of this disease, which he had seen, after vaccination, there were excellent cicatrices. Two persons in whom the vaccination had been perfect died; one of these individuals was 29 years of age.

Dr. COPLAND agreed in the main points with Dr. Gregory. Sixteen years since he had collected a number of cases to show that vaccination, however well executed, only afforded a temporary protection; that the protection for a certain time was perfect; imperfect after, and the imperfection increased in proportion to the period of time. The protection did not extend beyond the seventh or eighth year. At the ninth or tenth year, the small-pox would be modified, but unmodified generally at the 15th, 16th, 17th, or 18th years. He had published these facts in a work which he conducted at the time, but he was then considered as a false prophet. Time had not yet decided the influence that revaccination possessed over small-pox. In numerous instances in which he had attemped revaccination he had failed; in one case, in which he had tried the process three times unsuccessfully, the patient eventually contracted small-pox.

Dr. MOORE inquired of Dr. Gregory, on what ground he could positively assert that modified small-pox ever occurred. Had he ever inoculated a person from modified small-pox, and produced genuine small-pox?

Dr. YELLOLY said that in the celebrated "Fullwood's Rents Cases," matter was taken from a patient with modified small-pox, and this produced a similar disease in a person who was inoculated with it, and who before had neither been vaccinated or suffered from variola.

Dr. GREGORY thought that proofs that the mild disease following vaccination was really varioloid, multiplied on every side. He had occasionally, in persons very much averse to vaccination, inoculated their children for small-pox. For such cases he always preferred matter from the modified cases which maturated on the fifth or sixth day, and he found it answered well. We had other proofs, too, of the identity of the modified with the real small-pox. Persons every day contracted severe small-pox from those who laboured under the disease in its mildest and most modified form. Policemen for instance, in the same division, and on the same beat, would contract modified small-pox from modified cases; while, on the other hand, severe cases would produce modified ones in others. Regarding the number of cicatrices which he had found in persons suffering from small-pox after vaccination, he had seen one instance in which five very fine cicatrices existed. Generally speaking, however, there was only one; frequently two. Those patients who came from abroad had generally one on one of the arms; those who come from the provinces had one on each arm; those vaccinated in London had, generally, three or four cicatrices. In justice to the memory of the late Dr. Walker, he might mention that the largest proportion of modified cases, and modification, too, of the greatest extent, occurred in those cases which he had vaccinated.

Dr. MOORE said that the last speaker had failed to answer the question put to him. He (Dr. M.) had no doubt in the world that small-pox frequently followed vaccination; but he had seen many cases of what was called modified small-pox going through their course in five or six days, which he doubted much to be small-pox at all. Matter taken from these acuminated vesicles he had found only to produce a little irritation. Had Dr. Gregory ever known matter taken from a person affected with modified small-pox produce genuine small-pox in a person inoculated with the modified matter? He (Dr. M.) always made a practice of vaccinating a patient in one arm, and, at the end of twenty-four hours, inoculating him in the other. He found the vaccine vesicle went

through its regular stages, whilst the small-pox vesicle was suspended.

Dr. GREGORY said the case alluded to by Dr. Moore, in which a little irritation only was produced by the insertion of matter from varioloid disease, could only hold good with respect to highly modified cases, in which the matter was not sufficiently eliminated for inoculation. In those cases which maturated on the fifth or sixth day, matter might be procured on the fourth day which would produce small-pox.

CALCULUS IN SEAMEN.

A note was read from Mr. COPLAND HUTCHINSON, inclosing a case from Mr. Smith of Bristol. It was a case of stone in a patient who, twenty-five years before, was sounded, and a mulberry calculus detected; but, to use his own words to Mr. Smith, "instead of being cut, he cut the surgeon, and walked off to sea, where he remained eight years." He lost all pain, and all the symptoms of stone while at sea, but of late had been troubled with it again, and now requested to be operated upon. Mr. Smith was the surgeon who, twenty-five years before, had sounded him, and thought his tale probable from a variety of collateral circumstances. He was cut, and the stone removed. The case was brought forward by Mr. Hutchinson to know the influence of a sea life in preventing calculus.

Dr. JOHNSON said, that as the size of the stone, at the first examination, was not mentioned, there was no proof that the stone removed was not the original one enlarged.

Dr. WEATHERHEAD thought that the case brought forward proved nothing, except, perhaps, that circumstances occurring at sea diminished the sensibility of the bladder.

Mr. PERRY thought the case might be easily explained. The calculus, it would appear, was originally a mulberry one, which, of course, was the most painful form of stone. After some time a deposit formed around, and the surface became smooth. This would account for the decrease of the pain. He remembered a case of a child, under the late Mr. Earle, in whom a small calculus was detected. The child was treated by alkalies, and all pain subsided. It was at first considered that the stone might have been dissolved, but, on sounding, a calculus of considerable size was detected: lithotomy was performed. The stone was the size of a pigeon's egg, and smooth, but on being sawn through displayed a very irregular calculus, which had produced the irritation originally.

Dr. COPLAND inquired if it were not probable that the quantity of salt meat eaten by sailors might not have an effect in removing or preventing stone in the bladder. Perhaps the muriate of soda, while combined with the animal fibre, might form some of the other compounds of soda, such as the carbonate, and this might act powerfully on the disease.

Dr. JOHNSON inquired whether the people in the Hebrides, who lived much upon red herrings, were free from stone? What was the chemical explanation of muriate of soda becoming a carbonate when in contact with the animal fibre?

The PRESIDENT, on retiring, said that the Society on the present evening, had departed from its usual course; but from the interesting nature of the debate it was not to be regretted.

Busts of the late Dr. Babington and Mr. Earle have just been placed in the library.

WESTMINSTER MEDICAL SOCIETY.

Saturday, December 8, 1838.

Mr. HALE THOMSON, President.

DISEASE OF THE STOMACH FROM DISPLACEMENT OF THE ENSIFORM CARTILAGE.—INFLUENCE OF CERTAIN STATES OF THE WOMB OVER ORGANIC DISEASES.—DIABETES.

DR. BIRD related the following case, as offering, if not altogether unique, at least some remarkable facts:—A man, a short time since, presented himself at a dispensary to which he (Dr. Bird) was attached, having all the appearances of a person in the last stage of phthisis. He had a tallowy complexion, with large dark eyes and black hair, and he was remarkably emaciated. He suffered, however, from no cough, and careful and repeated examinations of the chest detected nothing wrong in that region. The history he gave was to this effect: he was a sawyer by trade, and usually worked at the upper handle of the saw, in consequence of which his body was being constantly bent forward. He was in the habit of wearing a leather belt very tight round the waist. About three years since he began to be harassed by the occurrence of vomiting immediately after taking food, which came up unchanged. After about two years the food was retained on the stomach until it was partially digested, and was then vomited up, with large quantities of tenacious and ropy mucus. At the time of application to the dispensary he suffered still from this vomiting, in addition to which he discharged a large quantity of semi-adipose matter from the intestines. On examining the region of the stomach no tumour was perceptible, but, from the general appearance of the patient, and the symptoms under which he laboured, scirrhus of the pylorus, or disease of the head of the pancreas, were suspected to be present. The evacuations of semi-adipose matter was a symptom of the latter disease, according to Dr. Bright. The usual remedies for vomiting, such as hydrocyanic acid, &c. afforded no relief; he got worse, and at the

end of a fortnight it was determined to examine him more carefully in bed. The first thing observable at this examination was an exceedingly elongated ensiform cartilage, which, bending inwards, pressed upon the neighbouring parts. When the patient placed his fingers under the displaced organ and elevated it, he was enabled to prevent the vomiting. In a week afterwards, on examining the cartilage again, it was ascertained to be as nearly as possible broken off. Since this had occurred the medicines, which before were of no benefit, had now a decidedly good effect; the occurrence of the vomiting was less frequent, and the patient was altogether improved. He was at present in Guy's Hospital, and was considered by Dr. Bright to be labouring under scirrhus of the pylorus. The case was curious, as the disease, whether it were scirrhus, or disease of the head of the pancreas, was, no doubt, brought on by the bending in of the ensiform cartilage.

Dr. JOHNSON had never seen a case of the kind, but he remembered a remarkable instance in which scirrhus of the pylorus was arrested by the occurrence of pregnancy. The patient was the wife of a medical man, and had suffered for some time under the symptoms of pyloric disease; a tumour was felt in the region of the pylorus; whatever food she took was very shortly after vomited up; she was much emaciated, and her countenance bore all the usual evidences of the presence of organic disease in the system. She became pregnant; for the first five or six weeks afterwards the vomiting was increased in severity, and she was altogether worse. During the last four or five months of her pregnancy, however, no vomiting had occurred, nor has it done so up to the present time, when she is suckling; on the contrary, the food is not only retained, but appears to nourish her, as she has attained a considerable degree of *embonpoint*. She had lately attempted to wean her child, but the sickness returned, and she desisted from the attempt. The organic disease of the stomach, no doubt, still remained.

Mr. D. O. EDWARDS said, that the case related was interesting, as showing the influence which certain states of the womb exerted in the arrest of organic disease. He would relate an instance which, though it did not enable us to lay down any general principle, would perhaps illustrate the above influence. A lady, 42 or 43 years of age, of very delicate health, and suffering from profuse menorrhagia, had been for two or three years under his care. About eighteen months since the existence of phthisis was suspected, and, on examining the chest, a vomica was detected under the left clavicle. Dr. Addison and Dr. Smith, of Reading, had examined the patient, and agreed as to the presence of the cavity. She was ordered into the country, and her regimen strictly attended to. It was considered that she could not last more than two months, but she was alive now, at the expiration of a year and a half; the disease did not seem to have at all progressed, but she remained much emaciated. She was free from night perspirations. The vomica was still to be easily detected; the affected lung was impervious. The right lung was free from disease, except, perhaps, the deposition of a few solitary tubercles. He (Mr. E.) could not account for the arrest of the progress of the phthisis in this case, unless it was from the womb affection; the womb itself did not appear to be diseased, but merely functionally disordered. In addition to the evidence afforded by the stethoscope in this case, of the presence of phthisis, the patient had all the usual symptoms of the disease, and it was also prevalent in her family.

A MEMBER did not consider that the arrest of the phthisis, in the case related, was dependant upon the womb affection; cases in which phthisis was arrested by the occurrence of pregnancy were very common, but these bore no analogy to the case in question.

Dr. A. T. THOMSON thought that the case related by Mr. Edwards offered a suggestion which should not be lightly passed over; it was well known that many years since repeated small bloodlettings were of much advantage in arresting the progress of phthisis—he had seen cases in point. The case of Mr. Edwards might, perhaps, have been influenced by the repeated losses of blood.

Dr. J. JOHNSON said, that vomica did not destroy life simply as vomica, but merely as forming the nucleus of an extension of disease. Small repeated bloodlettings, so much practised by the American physicians forty or fifty years since, would act almost as a specific in arresting the development of tubercles, and the consequent spread of the disease, by diminishing that inflammatory action, which he considered always existed in the neighbourhood of tubercles, and hurried on their development.

Dr. BIRD inquired if it were not probable that the case under discussion were not one in which tubercles were confined to one lung? If so, and the other lung were permeable, he did not see anything unreasonable in expecting that life would be continued for a long period. He had seen three cases in which this occurred.

Dr. STEWART knew a case in which many competent authorities had declared, ten years since, that a vomica existed in one of the lungs of the wife of a medical man, who was now living, and rather better than she had formerly been. The other lung was perfectly healthy.

Dr. A. T. THOMSON had at present a case of empyema under his care, in the North

London Hospital, in which the patient, a man, had, for the last fourteen months, discharged a quantity of pus from a fistulous opening in the side. The lung on the affected side was entirely collapsed, owing to the pressure of the fluid; the other lung was healthy. He considered in this case the pus came from the membrane exterior to the lung. The patient was improving, and the bulging out of the ribs becoming less prominent.

Dr. J. JOHNSON made some remarks on the mode pursued by a certain physician, in "curing" consumption! This physician instructed his patients to inflate and distend the lungs by mechanical expansion, by which force he (Dr. J.) believed the inflammation around the tubercles was increased, and their development, in many cases, hurried on. A more ridiculous, absurd, or unphilosophical plan was, he (Dr. J.) considered, never thought of. He had seen patients dying with consumption, whom that physician had, two or three days before death, buoyed up with promises of recovery. He (Dr. J.) might just mention that the case he had related a week or two since, in which the rectum had protruded into the vagina, was getting well under the means employed. There was now no doubt as to the exact nature of the case being as he had at first suspected.

Mr. H. THOMSON mentioned the case of a man who, two years since, suffered from empyema, consequent upon inflammation of the lung, on whom he operated, and took away two quarts of fluid, the relief afforded by which was extreme. Six weeks afterwards matter again formed, the operation was again resorted to, and twelve ounces of fluid removed. He had not regained the use of the affected lung, but the opposite one was healthy. He left the hospital in tolerable health. Four months since he again came into the hospital with all the original symptoms of empyema, and was anxious for the operation. It was doubtful whether this would be resorted to, circumstances having occurred which might prevent it.

A MEMBER alluded to a case of phthisis in which a man died from rupture of a large vessel, which had become implicated by the presence of a small vomica in the lungs. In this case the man was very stout, and had lost none of his flesh.

Dr. THOMSON mentioned a case of diabetes insipidus, in which the urine was diminished to a remarkable extent in consequence of the occurrence of considerable gastric inflammation, arising from the use of croton oil as a purgative.

Mr. EDWARDS had found grain-doses of opium, every three hours, successful in curing diabetes insipidus. His patient, while taking opium, had an enormous appetite.

Mr. HORNE had cured several cases by giving doses of 20 minims of tincture of lytta and 20 minims of liquor potassæ, every morning, and a large dose of laudanum in the evening, at bed-time.

Dr. JOHNSON said, that the tincture of lytta was generally given in too minute doses; it should be commenced in ten or fifteen minim doses, three times a day, and increased gradually, until half an ounce was given in the twenty-four hours.

Saturday, Dec. 15, 1838.

MORPHIA AND ITS DOSES. — POISONING BY SMALL QUANTITIES OF OPIUM. — WHAT QUANTITY OF OPIUM WILL PRODUCE DEATH? PARALYSIS OF THE PORTIO DURA.

Mr. CHANCE inquired what was the experience of members respecting the doses of morphia, and called the attention of the Society to a statement made by M. Raspail, and inserted at page 409 of THE LANCET, in which he states that one or two grains of opium will produce sleep, and a few grains more will cause death; that 29.5, and even 59 grains, of the acetate of morphia might be given or injected into a vein without causing death.

Dr. ADDISON deprecated the promulgation of statements so vague as that put forth by Raspail, he thought they were calculated to do mischief, and furnish an excuse for giving dangerous doses. He (Dr. Addison) had given ordinary doses of pure morphia, which was less soluble than any of its salts. He had known an instance in which a dose under a grain produced all the symptoms of poisoning. Raspail was a clever man, but bold in his statements, and not particularly cautious.

Mr. VERRALL said, that much would depend on the habit of the individual who took so large a dose as that mentioned by Raspail. Was the person an opium-eater?*

Mr. HORNE had found that persons in the habit of taking morphia, were, after a certain time, not only unable to increase the dose, but were obliged to diminish it.

Dr. WILLIAMS considered that the assertions made by Raspail were to be taken *cum grano salis*. He was undoubtedly a clever man, but liable, out of mere opposition, to make statements not altogether warranted. The mode of action of morphia would, he considered, render the quantity in question impossible to be taken with impunity. It acted directly on sensibility, and such a quantity would quickly destroy that of the nerves of respiration, and produce death. In this respect it differed from belladonna

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* The person was neither an opium-eater nor a drunkard; neither a Turk nor a Christian; but simply one of Magendie's *dogs*, who has thus unexpectedly excited so much interest in a learned Society.—ED. L.

had some other vegetable poisons. Belladonna, for instance, when given in large doses, such as a drachm of the extract, did not produce effects beyond a certain extent; and these effects did not seem to implicate the vital functions. He was not aware that any animals had been poisoned by belladonna, but cases were recorded in which small doses of morphia had proved fatal to them.

Mr. Snow thought that, as morphia did not contain narcotine, which is the most poisonous property of opium, it might be possible that Raspail was right. He was inclined to think that morphia acted in a similar manner to belladonna, and was a mere sedative. He recollected reading a case in which a dog was poisoned by belladonna.

Mr. Hale Thomson thought that every one who had had much experience in the employment of morphia, would at once perceive the impossibility of the truth of Raspail's statement. He had seen morphia administered in many surgical cases for the purpose of relieving pain, but never to anything like the extent mentioned by Raspail. In a case of acute *tic douloureux*, with violent action of the intercostal muscles, occurring in a man who had been in the habit of taking large doses of opium, he had, on one occasion, prescribed a draught containing six grains of morphia, and another containing four grains, to be taken two or three hours after, if the pain was not relieved. The draughts were taken according to the prescription, and he enjoyed a tranquil night. He had not since been able to take doses near as large, and he (Mr. Thomson) had found that this fact generally held good, that patients who had been debilitated by the use of opium were not able to bear doses so large as they had formerly been accustomed to.

Mr. Streeter inquired if it were not probable that there was a typographical error in the statement under discussion. In the "Confessions of an English Opium-Eater," he states, that in Buchan twenty-five ounces was printed for twenty-five drops. What was the smallest dose of opium which had proved fatal? He had seen a case in which an infant about three weeks old, was placed in much danger by the administration of half a grain of Dover's powder, which had been taken in mistake for half a grain of ipecacuanha. He recollected a case, also, in which a drachm and a half of the liquor opii sedativus proved fatal when given to an insane man; and another case in which, after a compound fracture of the thigh, twenty minims of the liquor opii sedativus proved fatal to an elderly female. In a case in which six drachms of laudanum were swallowed by a woman, she was saved, though with much difficulty; yet several months afterwards an ounce proved fatal in spite of the most prompt treatment.

Dr. Addison said, that Magendie had given a grain of narcotine mixed with oil, to a dog, and the animal perished. Dr. Roots had experimented with this preparation of opium, and had found it to possess simply tonic properties. Was it, then, poisonous to animals, and inert in the human system?

Mr. Chinnock had found acetate of morphia one of the most valuable preparations in some cases of hysterical monomania, and trusted that members would still continue to use this medicine in the usual doses.

Mr. Streeter was now called upon for a promised paper, entitled

ON PARALYSIS OF THE PORTIO DURA, ILLUSTRATED BY CASES; TO WHICH ARE PREFIXED SOME GENERAL OBSERVATIONS ON THE NERVOUS SYSTEM.

The author, in his preliminary observations, took a review of the progress of our knowledge respecting the nervous system, which, he said, till within the last half century, consisted of nothing worthy of the name of system, though isolated facts and ingenious conjectures had been contributed by many physiologists. To Dr. Gall was attached the merit of sustaining by proof the great idea which had been suggested by Willis, Charles Bonnet, Boerhaave, Haller, Van Swieten, Prochaska, Soëmmering, and some others, of the plurality of the organs of the brain, and the assignment of different functions to different parts of it. Aided by Spurzheim, he successfully applied himself to the elucidation of the functions of the brain, by long and laborious observations on the relative size of its various parts. The author then proceeded to notice cursorily the labours of Bell, Mayo, and Shaw, in this country; and those of Magendie and Panizza on the Continent; and considered that, through the labours of these physiologists he might affirm, that it was now generally, if not universally acknowledged, that any part of the frame which exercised various functions must receive as various sources of nervous supply. One nerve for motion, another for common sensation, and another for the enjoyment of a special sense. The author then made some remarks on the late researches of Dr. M. Hall, and said, that to all who labour to extend the limits of our knowledge, and freely communicate the result, the tribute of attention and respect was due; if the proofs as yet adduced failed to demonstrate the existence of this separate system as convincingly to the minds of others as they had done to his own; if further investigation should even confirm the opposite opinion, that as there is but one muscular fibre to execute the voluntary and involuntary movements of the frame, so there is but one nervous fibre to convey impressions to, and mandates from, the different, although intimately communicating nervous centres of voluntary and of involo-

luntary associated actions,—movements of irritability and not of consciousness,—and therefore not requiring cerebral existence or communication; they nevertheless seemed to him to establish claims to the extension of our knowledge of these phenomena, to an entire originality of their arrangement, to an attempt to elucidate the laws that regulate them, and to the merit of directing attention to the practical value of a knowledge of these phenomena, which, as well as their cause, were legitimate and worthy objects of research. Paralysis, in common language, had been applied to designate the loss of any power or function which was supposed to depend on any part of the nervous system. The kidney, the liver, the intellect, and the feelings, are said to be destroyed, or paralysed; but nosologists had generally wished to restrict its use to the loss of common sensibility and motion, and to the loss of the nervous endowment of the special sense, where the external apparatus of the sense remained perfect, and the nerve of special sensation has alone suffered lesion. On the present occasion he wished to direct the attention of the Society to that variety of paralysis in which the entire muscles of one side of the face were suddenly deprived of their moving power, usually spoken of as paralysis of the portio dura. The symptoms which marked this affection were too obvious, when once seen, ever to mislead; every movement of the face was gone, except those of the jaw which took place in mastication; there was usually lachrymation; but one of the most prominent symptoms was the inability to completely close the eyelid; if permanent, this was a serious occurrence, as it finally led to destruction of the eye itself from continued exposure. Mr. Streeter then proceeded to say,—

When the disease has long existed, the sympathy of the two sides of the face is also destroyed from the wasting of the muscles of the one side, and their increased strength on the other, so that the nostril is contracted and the mouth distorted. The one only sign necessary for its diagnosis is the tranquillity of the paralysed side, compared with the exaggerated expression of the opposite, on the persons putting these parts in action, as in speaking, smiling, &c. Various divisions of the cases have been made, according to the origin to which it has been assigned. I shall enumerate three classes, though, from what I have seen, I am inclined to think the two first differ more in the degree than in the nature of the pathological condition of the nerve on which they depend. The first form is that arising from exposure to cold. The earliest notice I find of it is in the writings of ophthalmic surgeons. The imperfect power of closing the eye, lagopthalmos, as it is termed, is stated to frequently originate from temporary palsy of one half of the face, produced by exposure to a current of air. Dr. Monteath, the translator of "Weller's Manual of Diseases of the Human Eye" (1821), from observations on several cases, is convinced that cold is its invariable cause, determining at first an inflammatory state of the pes anserinus, and perhaps, in some cases, an inflammatory swelling and diminution of the calibre of the aquæ ductus Fallopii, causing pressure on the trunk of the nerve. Pain, he says, for some time radiates from the ear along all the branches of the nerve; it then ceases, and perfect semi-facial palsy follows.

I myself became acquainted with it during my attendance on the lectures of Mr. Tyrrell, at St. Thomas's Hospital, on diseases of the eye, in the spring of 1825. He described it as occurring in combination with paralysis of the eyelids, and spoke of the importance of being aware of its seat and cause; "for (said he) we are so often seeing paralysis connected with cerebral affection, that we might associate the idea of cerebral disease, which, in reality, does not usually accompany it."

Sir C. Bell, in his paper in "Phil. Trans.," 1821, says, by knowing the distinct functions of the two nerves supplying the face, the physician will be better able to distinguish between that paralysis which proceeds from the brain, and that partial affection of the muscles of the face, when from a less alarming cause they have lost the controlling influence of the respiratory nerve. Cases of this partial paralysis must be familiar to every medical observer. It is very frequent for young people to have what is vulgarly called a blight, by which is meant a slight palsy of the muscles of one side of the face, and which the physician knows is not very formidable. Dr. Johnson has also recorded a case of unusual severity and duration (1825), in his Journal, occurring in a medical man; three cases of this kind have come under my own care, which I will briefly narrate.

The first was in April, 1829. A gentleman, about twenty-six years of age, whose health was much impaired from injudicious application to study; he had been exposed to a draught of air from sitting near an open window, and on getting up one morning soon after was surprised to find that he could only smile on one side of his face. The treatment consisted in leeching, the application of fomentations in the first instance, with suitable constitutional remedies, and subsequently of the application of a small blister, and the compound camphor liniment. He recovered in two or three weeks, and informed me, in a note received during the week, that he has remained unaffected up to the present time.

The second, is somewhat curious, from occurring in a woman only the day before she

was delivered of a child. She was considerably alarmed at its occurrence; could trace it to no particular exposure, although she was constantly subjected to cold in her occupation, that of sitting at one of the open sittings of Covent Garden Market, selling fruit, in which her husband is a dealer. She recovered with the usual discipline of the lying-in chamber, and the application of a blister to the side of the maxilla, much quicker than the former or subsequent case. It occurred in July, 1835, I have seen her today, and she says she has had no return.

The third occurred in the early part of last spring, in a person well known to many members of this Society, Mr. John Miller, so justly celebrated for his minute dissections, and his skill in putting up anatomical preparations. He had been incautiously dissecting near an open window during the cold weather; he also awoke and found himself semi-paralysed, and came to me in great alarm. I satisfied him that there was no great reason for it; that he would soon get well. Blistering and slight constitutional treatment were followed by an early disappearance of the symptoms.

The exact state of the nerve during the loss of muscular power is one of some interest generally. Does it depend on inflammation, or is it occasioned by pressure from effusion the result of inflammation? Are there any facts which prove that inflammation is ever an immediate cause of paralysis in nervous structure? As far as I am aware there are none, unless that inflammation goes on to the production of effusion and mechanical pressure, or complete disorganisation of the tissue. A second form of the affection results from inflammation in the ear, leading to abscess, involving the canal through which the nerve passes. This is described, for I have not seen such a case in its early stage, as a much more permanent affection, and as one which is seldom cured. If threatened, it would demand a vigorous and active treatment. In one only, out of four cases, related in a valuable paper on this subject by Mr. Shaw, in Vol. 12 "Med. Chir. Trans.," did recovery take place.

Other cases will be found recorded by Dr. Bellard, in the "Archives Géneralés," Nov., 1824; and by Sir Charles Bell, in his "Cases and Consultations."

The third and last class of cases which I shall mention are such as result from the pressure of tumours on the nerve—necrosis of the bony canal,—its implication in malignant ulceration of the cheek, of which I have seen an instance, and its accidental, or unavoidable division in wounds by operations. Of these I shall only detain you to remark, that it will be interesting to inquire if recovery has taken place in any case after it has been divided. Although it is to be expected, I am not aware that any such has been observed.

Dr. Addison had been often struck with the analogy which seemed to exist between the disease mentioned in the paper of Mr. Streeter, and rheumatism in its various forms, when produced by cold, and thought the effect produced on the nerves of sensation and motion were the same in both cases. Thus, for instance, we saw paralysis of one or both extremities occurring as a sequence of rheumatism produced by cold, or it might take place and rheumatism not be present. In the same way we might have neuralgia of the sciatic and other nerves; and, again, the paralysis might be the precursor of the rheumatic affection; and he had lately seen a case in which paraplegia was followed by acute inflammation of the knee-joint. These cases being treated as common rheumatism got well. Tetanus, as was known, might also be produced by cold, but this form of the disease yielded to remedies, which was not the case with traumatic tetanus.

Mr. Barlow, after some observations on the value of Dr. Hall's researches, said that Mr. Streeter had remarked, that the existence of the fibres on the functions of which the excito-motory actions were supposed to depend, had not been rendered evident to him, and that as there was one set of muscular fibres for voluntary and involuntary motion, so for the production of these opposite kind of movements only one set of nerves were necessary. He, Mr. Barlow thought a physiological fact was as capable of being proved as an anatomical one. When it was shown that after the cerebrum was removed, upon the application of stimuli to the extremities, the sphincters, and the larynx of an animal, convulsions were occasioned, the sphincters contracted and the glottis closed; that these phenomena could be produced as long as the spinal marrow and its nerves remained entire, but ceased on its destruction; it needed no unravelling of nervous filaments to prove that the spinal chord was necessary to, and an agent in, the exciting of such effects. In describing the symptoms of paralysis of the face, the author had not observed that, in cases where all voluntary motion was lost, and involuntary action remained in the instances where the disease was dependant upon cerebral lesion, the touch of an eyelash would close the eyelid, though the will could not influence it at all. This was not a cerebral, a voluntary, it was a spinal or excito-motory act. He had seen a case of paralysis of the face which occurred suddenly during parturition. There was reason to suppose that it resulted from a rupture of the cerebral vessels.

Dr. Marshall Hall said, he was taken quite by surprise to find the subject of the discussion one in which he was so deeply and personally concerned. He thought the sketch read before the Society, by the author, very able and impartial; and

he was quite gratified by the notice with which he had honoured his (Dr. Hall's) researches. If he had a fault to find, it was that, like so many others, he still expressed a *doubt* on a subject which, in the manner in which he had treated it, was almost entirely the detail of *experiments*. "My publications," said Dr. Hall, "*consist*, indeed, of little more than of such a detail, and to doubt, is not to doubt my views, but our means of investigation; it is to doubt whether there be such a *science* as *physiology*." He would ask the able author, whether he ever saw thorough paralysis of the *facial* nerve, or of the orbicularis palpebræ, the special subject under our notice, in hemiplegia,—that is, from disease of, and confined to, the cerebrum? And he (Dr. H.) would ask, whether he has not seen the experimental facts that division of the trifacial or of the facial nerve, or of the medulla oblongata, does respectively paralyse the function of the orbicularis, as connected with the touch of the eyelash; and that the destruction of the function of the facial nerve by the pressure of a tumour, or by disease, paralyses this muscle altogether? The very subject of this night's discussion proves that there *is* a distinct cerebral and true spinal system. When in Paris, two years ago, he (Dr. H.) saw M. Magendie, and said to him, "You have divided the *trifacial* nerve within the cranium in rabbits." M. Magendie said, "Yes; and I will present you with the instrument by means of which it was done." "But," I said, "how did you *know* that this nerve was divided?" He replied, "I know it, because the eyelids then fail to close when their border is touched." Every one knows the fact of this action being annihilated at once by division of the facial nerve, and by injury of the medulla oblongata. Here, then, we have the demonstration of the system in a detail of facts known to all. Where is the room for doubt? It is no hypothesis of mine. This, and similar statements confined to details of experiments, are facts,—obvious facts. But it has become the fashion to criticise and doubt. The system is, in one page of a pompous, but untruthful journal, described as being *old* but *good*; in another page even of the same number of that journal, as *new* but *good for nothing*; and youthful critics, by name, and anonymously, have sacrificed truth on the demon shrine of falsehood and malice. But he must beg pardon for occupying the time of the Society with these things. If what he (Dr. Hall) had done be new and useful, let it be accepted by his brethren; if otherwise, let his errors be kindly and gracefully exposed. Hitherto he had met with such treatment as had often made him wish that he had never seen a nerve, or entered upon their investigation, however interesting.

Mr. Snow regretted that Mr. Streeter had spoken of the excito-motory system as something which still had to be proved, and considered that any one who had read attentively the works of Dr. Hall, Dr. J. Reid, M. Flourens, and others, must feel convinced of the truth of the system, and that the late discussion referred merely to the merits of the different authors as originators of the views advanced.

Mr. Streeter agreed, in the main, with the observations of Dr. Addison. He was aware that in hemiplegia paralysis of the eyelid did not exist as a symptom. The objections he had raised to the general views advanced by Dr. Hall, arose from his idea that separate fibres existed for voluntary and involuntary movements, and for incident impression and conscious sensation. He objected, also, to the conclusiveness of experiments in the performance, as much injury was done to neighbouring parts. He looked rather to pathology than experiment for further demonstration. Had Dr. Hall met with further proofs of his views from comparative, but particularly insect, anatomy?

Dr. Marshall Hall rose to make a few brief remarks in reply to what had fallen from Mr. Streeter. Mr. Streeter had spoken of his (Dr. Hall's) views of distinct cerebral and spinal nervous fibres. Dr. Hall said that he had not written ten lines upon this subject; that he had purposely avoided it, not because that was not his *opinion*, but because the *proof* was difficult, an observation which does not apply to his true spinal or excito-motory system; for this is demonstrably established by experiment. But, if the subject was to be discussed, Dr. Hall would ask what would be said of the olfactory nerve? Was that not distinctly *cerebral*? And he might ask, what was the trifacial and facial, as the excitor and motor in the closure of the eyelid when the brain is removed? M. Flourens and Professor Müller contend that volition acts upon and through the medulla oblongata,—to use a figure of the latter eminent physiologist, *as the touch upon the harpsichord*. Now, if this were true, some phenomena could not occur which we see daily in practice. In *chorea* the function, if not the structure, of the spinal marrow is morbidly affected. Now, if volition act on this organ as the finger upon the harpsichord, and if this organ be so *out of tune*, how could *harmony* in the movements *ever* be produced? The energy of the touch would not put the instrument, and "its ten thousand strings," into tune. But, although we see that the patient affected with chorea cannot *walk*, except with disorderly movements, yet he can run with regularity! This is, indeed, an argument, not from anatomy, but from physiology or pathology, and it has become fashionable to dispute everything which is not anatomy; but are physiological arguments of no value? Really

it appears, then, to disprove the opinion of M. Flourens and Professor Müller, and to be in favour of that opinion which appeared to the speaker to be most palpable, and to point to distinct voluntary or cerebral fibres, and fibres from the true spinal marrow. He would again say that he stated this as mere opinion, and would repeat that he did not regard it as established in the ample and complete manner of the excito-motory system, the system of the vis motoria, and (not of the "sympathetic actions," which phrase is the resort and the way of escape of his critics, but) of the system of ingestion and egestion of the orifices and sphincters, as well as of the *whole class of spasmodic diseases*. He (Dr. Hall) again begged pardon for the length and tone of these remarks. They had been "excited" by the unfriendly treatment which he had received, in high places and low, in reference to his researches, which, founded, as they are, upon facts, nay, being, as they are, the expression of facts, have been misrepresented and distorted with an ingenuity which might have been much better employed, and with a malice which has blotted a fair page in the history of physiological science.—The debate was adjourned.

MEDICAL SOCIETY OF LONDON.

December 10, 1838.

The Society was occupied this evening in a discussion on tumours, arising out of a case related by Mr. Pilcher. The case occurred in a man whose family are scrofulous. The disease commenced seven or eight years since, without any apparent cause, though it appeared that the part was struck by a handspike some years since. The tumour had apparently commenced in the medullary structure of the inner condyle of the left thigh; at first very small and firm, and without pain. It continued to increase in size gradually, but slowly, and two years since had acquired the size of a pint basin. Amputation was then recommended, but declined on the part of the patient. The man's health was good.

Mr. Pilcher lost sight of the case until two months since, when he found that the tumour measured thirty-four inches round, and occupied the fore and inner part of the thigh. He had still no doubt about the propriety of amputation. The leg was removed, and the patient did well. The leg after removal weighed thirty-four pounds and a half. During the last week or two before its removal the patient complained that the tumour pained him occasionally at night. It was a well marked case of exostosis cellularis.

Monday, Dec. 17, 1838.

MALFORMATIONS AND THEIR CAUSES.

Mr. Bryant, the President, laid before the Society a fœtus, apparently of about the seventh month, which presented the following remarkable deviations from the normal formation:—

The abdominal muscles were wanting, and the viscera were covered merely by a thin, transparent membrane. The lower part of this membrane was integumental, with a crescentic edge, above which the openings of the urinary and digestive canals might be seen. The umbilical cord had but one artery, which went to the right internal iliac, the vein terminating near the edge of the liver. The head was well formed. The lower part of the vertebral column was deficient in its spinous processes, and the sacrum altogether wanting, with a bulging of the serous coverings of the medulla spinalis into a small bag, containing the fluid of the spinal column. The heart and lungs appeared to be well formed. The pharynx, œsophagus, stomach, and small intestines, appear to be naturally formed, but that which would seem to correspond to the ileum, terminated in a very minute opening, just above the crescentic integumental edge above described, or midway between the situation of the umbilicus and the pubis. The large intestines were wanting, only a portion, answering in appearance to colon, about two inches in length, both ends of which opened near to the ileum, the opening being about the size of a shilling, and well defined. The kidney on the right side occupied its usual position, with its supra-renal capsule of its customary size and appearance. The excretory duct, the ureter, passed downwards towards the pubis, and ended in a very minute orifice somewhat below the opening of the small intestines; a bristle might be passed through it. The left kidney with its supra-renal capsule of natural size; the pelvis of the organ was very large, and extended into a very considerable bag, analogous in its structure to the bladder; this bag was divided, as it were, into compartments (the right ureter was attached to the right side of this pouch), and was distended with fluid having an urinous odour. The testicles were of their usual size in the abdomen. A sort of rudimentary penis, with a scrotum, was observable, as was also a short urethra, opening above, under the crescentic edge of integument.

A conversation followed the history of this case, respecting the causes which influence the formative process of the fœtus in utero, and also as to the effect which the mental impressions of the mother had over the development of her offspring. If, as many believed, the imagination of the mother would so influence the child as to

produce arrest of development, or a malformation, similar to the cause which produced it, at what period of utero-gestation did this influence cease to exert itself? Did "quickening" have the effect of preventing this cause taking effect? It was contended, on one hand, that the imagination of the mother did affect the formation of her offspring; but the speakers differed as to the time at which it exerted itself. One contended that there could be no doubt that at the period of conception, and in the early stages of pregnancy, this cause was in action, and the Bible was appealed to in support of the former idea, the illustration of Jacob holding the wands before the ewes during copulation being advanced in evidence. Certain circumstances, such as fatigue, for instance, at the period of coition, would prevent the full development of the future being; circumstances also seemed to exert themselves at many successive pregnancies, in which the same malformation was, in every instance, produced; and a case was related in which a woman was delivered of seven children successively, each of whom had a supernumerary finger, perfect in every respect, except that of the osseous structure, and appearing to have been coeval in development with the rest of the body of the child. Another speaker was inclined to limit the period at which the mind of the mother influenced her offspring to the time of "quickening;" but cases were related which were considered, at the least, as remarkable coincidences, in which singular malformations occurred, supposed to be from fright, though the pregnancy had advanced to the sixth month; and in one of these, that of a hare-lip, the woman herself was satisfied, long before delivery, that her child would have this malformation. A case was also related in which two sisters had children by one man, and every one of these children were club footed.

On the other hand, it was argued that the malformations in utero did not depend on any influence the imagination of the mother exerted over her offspring; that the cases in which such malformations followed frights were as drops in the ocean, compared with those cases in which malformations after frights did not occur; that mothers only attributed the malformation to a certain fright, or impression on the mind, after the child was born, and the mark, or deficiency, was observable; that all malformations might be classed under certain heads, Nature always following some determinate order in their production; that, for instance, hare-lip always occurred in the upper lip, and on either side of the median line; that all cases of perforation of the rectum, which were hitherto known, with one exception, might be classed under certain distinct heads; that the varieties in the arterial system, though numerous, were always uniform; that, for instance, if the vertebral artery ever deviated in origin, it was on the right side; if the ulnar artery arose from the upper part of the brachial, it was almost always found above the fascia; that in cases (as in the instance of hare-lip related) in which the fright was considered as the cause, the lip, in fact, must have been perfectly developed at that period, had no deficiency been present in the earlier periods of utero-gestation; that the period called "quickening," could not be taken as a limit to the influence of the mind of the mother, if any influence was exerted, inasmuch as this phenomenon was very variable as to the time of pregnancy at which it occurred, and that it was, in fact, rather a fancied than a real occurrence, many mothers never experiencing it, and those who did, "quickening at whatever time they please, but never on a holiday;" that our knowledge on the subject was undoubtedly imperfect in the highest degree; but that, though all the processes of nature in the production of malformation appeared discordant, that they were really "harmony not understood;" and that the future collection of facts might enable us to lay down such general principles as would materially assist us in the inquiry. It was also contended, that as no nervous connection could be traced between the mother and the fœtus, no direct impression could be conveyed to it; that depressing mental causes, by lowering the bodily strength of the patient, might also arrest the development of the offspring generally, but not in any particular part of the body,

The Society adjourned till the 14th of January, 1839, on which evening a paper will be read on the Spontaneous Growth of Nævi subsequent to Birth.

MEDICAL LEGISLATION IN JERSEY.—From the "Jersey Patriot" we learn that the question of medical reform is much debated also in that quarter of the British dominions. The Legislature of Jersey has "it in contemplation to pass a medical project which is now lodged *au Greffe*," for the regulation of medical practice in Jersey; but this measure is strongly objected to by some practitioners, on the ground that its enactment would "operate directly in opposition to the Apothecaries' Act of England, by *disqualifying* several persons who are entitled by that statute to practise, and *legalising* others who are expressly declared by the Act of 1815 to be unqualified." At present many practitioners are fulfilling medical duties in Jersey under the protection of foreign diplomas, and the "regularly qualified" men urge the presentation of petitions to the State for the extension of English medical laws to that island.

THE LANCET.

London, Saturday, December 22, 1838.

We are glad to find that the death of James Trickey, in St. Michael's Church, continues to excite discussion. It is evident, from the contradictory statements of medical jurists, that the physiological effects of different proportions of carbonic acid in the atmosphere are ill understood; that the way in which the products of combustion extinguish life, is a problem not yet solved; and, consequently, that the cause of James Trickey's death is involved in obscurity. All these questions merit investigation; but another important practical question entering into the inquiry, appears to us satisfactorily set at rest.

Upon the 12th of May last we spoke of the stove which James Trickey died in watching, as "the Jerusalem Coffee-house "bubble, Joyce's charcoal arcanum, and "patent chafing-dish for heating apart-"ments economically, and poisoning the "inmates gratis, with the products of com-"bustion." Now this, we admit, was strong language to apply to a new stove, for which a Government *patent* had been recently granted. It appeared something like an ill-natured attempt to decry a new invention. It seemed harsh treatment of the patentees. To show that no such feelings actuated us, and to evince a willingness "to try all things," even as far gone in absurdity as the science of Mesmerism, we inserted certificates from Messrs. Cooper and Brande, respecting the Jerusalem Coffee-house stove, which ascended the horizon nearly in the same *sign* (the Bull we presume) as Mr. Murphy's Weather Almanack, and succeeded in dividing public attention with that luminary. It is high time to lay before the public the reasons which led us to denounce this destructive invention; and in doing so we shall steer clear of the chemical disputes and refinements with which the simple question has been complicated. Is the burning of charcoal, in an inhabited room, dangerous to human life? The patent stove is a modification of the chaffer. The prepared carbon burns at the expense of the air of the room; the products of combustion are diffused through the chamber, and allowed to escape as they can through the crevices of the doors and windows. The fact to be determined in this case is, not whether 2 or 5, or 20, or 50 per cent. of carbonic acid, can be borne with impunity—whether this or that principle is fatal—but whether experience has established that the atmosphere of a room in which charcoal is burning is innoxious? Is it necessary, in the present day, to ask whether charcoal fumes are injurious, after thousands of deaths have been recorded from that cause? Are not charcoal fumes resorted to by suicides in Paris as frequently as the pistol or the dagger? It is well known that wood is burnt by the savage in his tent with a certain impunity. The smoke warms his body and escapes, and the fresh air enters in sufficient quantity to sustain life. Common chimneys carry off the carbonic acid, other gases, and empyreumatic oil, as well as a considerable proportion of the heat evolved in combustion. The smoke which escapes up the chimney is a poison, but it warns its victim, who can only perish by an act of desperate resolution; but wood will burn, produce heat, and destroy life, when it is reduced to charcoal, and deprived of the principles which announce its dangerous nature. Mr. Joyce has masked the poison; he has succeeded, it appears, from the analysis of Gay-Lussac, in manufacturing tolerably pure carbon, a combustible without any very disagreeable odour, but quite as inimical to life as the combustion of coal or cokes. Upon theoretical grounds we anticipated this fact; and Mr. Coathupe's hardy experiment, and fortunate escape, prove it to direct demonstration, for this ingenious gentleman must be looked upon almost as one come from the dead to witness to its accuracy.

But the *patent* stove has been employed with impunity in many instances. Yes, and charcoal has often been burnt in apartments with the same impunity; but when the apertures are partially closed, when any accident shuts in the poison, or renders the inmates susceptible to its baneful influence, death is not less certain; of this the case reported last week by Mr. CHAPMAN is an example. The poisonous effects of charcoal were not discovered for many years; choffers were burnt in apartments, and increased the sudden deaths and apoplexies, without leading to their suppression, until chemistry threw light upon the matter, when they were banished from use everywhere except in obscure country villages which were never visited by a newspaper. Now a stove, exactly similar to the open chafing-dish in principle, is audaciously advertised. An enlightened English Government grants the *discoverer* of this precious machine a patent to protect his rights! With the same *nonchalence* it grants patents to quacks convicted of manslaughter! In Paris the following announcement is distributed every year by the police; the document is given at full length by M. MARC, who remarks that suicides often resort to the fatal fumes of charcoal:*—

"13th Oct., 1823.

"CAUTIONS GIVEN BY THE COUNCIL OF HEALTH UPON THE DANGERS ARISING FROM CHARCOAL FUMES.

"*Many persons believe that charcoal may be burnt in a chamber*, or other close place, *without danger to health or life*, and that the smoke of coal only is injurious.

"*It is a fatal error*, which it is so much the more important to dissipate, as it every year costs several persons their lives, and that last winter, particularly, it gave rise to several accidents.

"1. A person who exposes himself to the vapour of lighted charcoal, runs the same risk as if he exposed himself to the smoke of coal; in other words, the emanations from burning charcoal cause death almost as promptly as those arising from coal.

"2. Consequently the practice of lighting charcoal, and allowing it to burn more or less in a vessel placed in the middle of a room—[as in Joyce's stove]—*is attended with the utmost danger.*

"3. Even when, from peculiar circumstances, which it would take too long to enumerate, this imprudent step does not produce immediate death, it may nevertheless *give rise to severe and often fatal maladies.*

* * * *

"5. Hence to attempt to heat inhabited rooms without chimneys, either with charcoal or with coal, is to expose ourself to *imminent peril.*"

These facts are popularly stated, but they express the general opinion of physiologists. What, then, are we to think of the rash evidence given by Mr. COOPER, at the coroner's inquest, who was decidedly of opinion that the air from the stove could not have affected JAMES TRICKEY's life—who could say this after Mr. COATHUPE's communication in THE LANCET, and add that the vapour would not affect a person asleep—and, moreover, that he (Mr. JOHN THOMAS COOPER) had respired at least 4 per cent. of carbonic acid gas (how long?) and was of opinion that the jury-room at that moment contained nearly that amount!! All those who have any acquaintance with Parliamentary Committees, where the interests of commercial bodies, such as railway companies, are concerned, are well aware that certain engineers, chemists, and physicians in London, are always ready to give strong opinions upon doubtful subjects. But nobody ever surpassed the reckless Mr. COOPER in audacity of assertion. Was Mr. COOPER upon his oath? It is well known that in the ancient Saxon jurisprudence it was customary to bring compurgators into court, who knew nothing of the facts, but simply swore that they believed the witness upon his oath, and that where a high degree of veracity was sought, 300 compurgators were sometimes brought forward. We confess that a larger number would never lead us to credit any chemist whom Messrs. HARPER and JOYCE might henceforward induce to declare charcoal fumes innoxious.

Mr. JOYCE, the patentee, stated that he had been fourteen years in bringing the invention to perfection; and the result was, that he had been enabled to remain in a room

* "Nouvelles Recherches sur le Secours à donner aux Noyés et Asphyxiés," par C. C. Marc, D M., 1835.

heated to 89°, without escape of gas. This is a strong proof of Mr. Joyce's vital tenacity, but not creditable in the slightest degree to his judgment. He offered to place his mouth over the tube of the stove for *five minutes*. We recommend him to make no such attempt, much less to sleep in a close room heated by his stove all night. If he did not invent, he might introduce, a new mode of committing suicide in this country; and suicide, Mr. Joyce, is out of the question; your case is not yet so desperate; your stove has done much mischief, but we are willing to believe that you sinned in ignorance, and that, therefore, you are not yet morally answerable for any injuries which it may have inflicted. Let not your conscience drive you to your own stove in a fit of self-destruction. At least, let Mr. Cooper place his mouth over the tube first.

Again, Mr. Joyce has a *great number of ladies and respectable families, now living*, ready to come forward and substantiate the fact that the stove is quite innoxious, from the nightly use of it in their bed-rooms. These zealous ladies should waste no time in bearing their testimony to the innoxiousness of the patent stove. If Mr. Joyce be the prudent man we take him to be, he will not lose a day in obtaining their written evidence, for who knows what a night might bring forth where the Jerusalem Coffee-house stove is in use? Charcoal fumes are exceedingly capricious things, and some fine evening they might betray all their malignity. Ladies in France, if we can believe the novels and police reports, often resort to the charcoal stove in paroxysms of disappointed love; whether the parties who granted Messrs. Harper and Joyce's patent had an eye to this ingenious application of the invention, or not, it is impossible to determine, but well-grounded doubts may be entertained whether their utility in such instances can justify the favour in which the destructive stove is held by Mr. Joyce's female partisans in England.

The families that persist in the use of Joyce's stove should give notice of their intentions to the *Humane Society*, and, if the officers of that excellent Society act with promptitude, they may probably restore some victims, to swell the grateful train at the next anniversary dinner.

The facts did not enable the jury to decide positively upon the cause of James Trickey's death; but we must make an observation on the turn attempted to be given to the medical evidence. It was stated in the careful record of the *post-mortem* examination, published by Mr. Green in this Journal, that the brain was greatly congested, and that there was a considerable effusion of serum both in the ventricles and under the arachnoid membrane. It was inferred that the patient died of serous apoplexy; but if this had been the case, does it follow that the congestion in the brain was necessarily spontaneous, and not the effect of the mephitic emanations from the stove? Certainly not. All authorities agree that this precise state of the brain occurs after poisoning with charcoal fumes. We have turned to several. We quote a passage from Christison's work on Poisons, which is in everybody's hands: —"According to Portal the *vessels of the "brain are gorged, and the ventricles contain "serum*, in persons killed by charcoal "vapour. * * Mertzdorff has related "a case of death from the same cause (the "servant had neglected to open the flue-trap "when she kindled the stove in the bed-"chamber), in which, together with the pre-"ceding appearances, an effusion of blood "was found between the arachnoid and pia "mater over the surface of both hemispheres. "This, so far as I know, is a solitary case "of the kind; but such appearances might "be expected more frequently, considering "*the manifest tendency of this kind of poisoning "to cause congestion in the head.*" Now, taking these facts, in connection with the statement of Dr. Bird, that he actually felt great inconvenience for some time after stooping down to collect the deleterious gas, and the declaration of Mr. Harris, the churchwarden, that on entering St. Michael's Church at 4 o'clock, he perceived a vapour

and felt a giddiness in the head, which was very violent,—there must ever exist a strong suspicion that the death of the unfortunate man was the effect of JOYCE's stove.

In the present instance we do not wish to push the argument further; we have evinced our sense of Mr. JOYCE's indefatigable fourteen years' labour by dissuading him from a rash offer of self-destruction; and, as we have recommended him not to kill himself by testing the perfection of his invention, we shall, in parting, give him another piece of good advice, and that is, not to kill other people; for the wealth which may be realised by the patent will furnish a sad anodyne to the unappeasable pangs of a troubled conscience. The patentees, we say again, must be acquitted, in the case of TRICKEY, of any fault but that of ignorance. Should another death occur, in similar circumstances, after this warning, they will be guilty of ———.

Principles of General and Comparative Physiology, &c. By WILLIAM B. CARPENTER. Churchill, London, 1839.

THE nature of Mr. Carpenter's work prevents us from attempting anything like an analysis of any portion of its contents. We must, therefore, confine ourselves to a simple announcement of its object, and a general opinion concerning its merits.

The object which Mr. Carpenter has had in view, in the composition of his Principles of Physiology, was to lay before the reader a concise, but at the same time sufficiently complete, sketch of the science, embracing whatever general principles may be regarded as firmly established, and illustrating them fully by a constant reference to the structure and functions of the different classes of animals, and of vegetables.

We are bound to say that Mr. Carpenter has succeeded in fulfilling his object in a most satisfactory manner. His work abounds in facts of great interest to the physiologist, which, if not new in themselves, are rendered novel by the way in which they are presented to the reader; the style throughout is easy and agreeable, the arrangement judicious, and the principal facts are illustrated by neatly-executed woodcuts or engravings.

The Medical Pocket-Book and Almanack for 1839. By JOHN FOOTE. Renshaw, London, 1839.

THE student will find this to be a very useful little pocket companion, superior to nine-tenths of the diamond manuals in which the medical press of the present day is so fertile.

In addition to the usual information respecting the different Colleges and examining bodies, it contains some useful selections from the French Pharmacopœia; an account of the methods employed for the formation of artificial mineral waters; a notice on poisons; and a few of the principal formulæ employed in the treatment of infantile disease.

A Manual of the Diseases of the Eye; or, Treatise on Ophthalmology. By S. LITTLE, M.D.; revised and enlarged by HUGH HOUSTON. Churchill, London, 1838.

HAD this been a simple reprint of Dr. Little's "Manual of Ophthalmology" we should have felt much pleasure in recommending it to our readers as a concise and useful treatise on diseases of the eye; but we are astonished that the respectable publisher did not perceive that the edition was got up for the express purpose of bespattering with praise Michael Ryan, Esq., in the execution of which laudable task the original work has been most disgracefully disfigured. Thus, Dr. Michael Ryan is quoted as an authority upon diseases of the eye no less than *twenty-five* times, while the names of Guthrie, Wardrop, Tyrrell, Lawrence, &c., occur about six or seven times. As to such ignorant (in comparison with Michael Ryan) opthalmologists as Scarpa, Beer, Ware, Alexander, Velpeau, Langenbeck, Wenzel, &c., they are mentioned about once or twice. The work is dedicated to Michael Ryan, *obstetrician*, and the "revisions and enlargements" were probably furnished by himself. If the reader, then, be anxious to ascertain a midwife's ideas concerning ophthalmology, let him forthwith procure this Manual; but if he be not extremely curious, we would advise him rather not to read at all than waste his time in perusing Mr. Houston's *blarney*.

ON

LUNATIC ASYLUMS AND ESTABLISHMENTS FOR INVALIDS

IN THE

North of Germany and in St. Petersburg.

BY M. LEURET.

(Continued from page 450.)

HAMBOURG.

The lunatics at Hambourg are all sent to the General Hospital. I augured ill from this circumstance. In a general hospital the lunatic patients are generally thrown into any corner of the establishment, and seem as if they were received for the purpose of being got rid of, and not treated. My fears were but too well founded. To prevent these unfortunate beings from disturbing the other patients by their cries they are thrust into the *cellars*; these cellars, indeed, are clean enough, and comfortable, but the light of the sun can never reach them, and the unruly patients are thus confined in perpetual darkness. There are numerous methods of ill-treating insane persons, but I had been unacquainted with this one, which I regretted extremely to witness in the rich and flourishing city of Hambourg; the blame rests with the inhabitants of the city at large. The Senate, composed of twelve doctors, and twelve merchants, regulate the municipal affairs of the city, the expenses, &c. This body proposed to the Assembly of Commoners, that a sum should be raised for the purpose of erecting a special hospital for the insane; the project was rejected, and the corporation of one of the wealthiest cities in Europe refused to raise a few thousand pounds for the benefit of their insane brethren.

The cellars of the Hambourg Hospital are also destined for the reception of patients affected with hysteria, epilepsy, and such other disorders as require seclusion. Generally speaking, the wards of the hospital are small, but they are well ventilated, and each is furnished with a water-closet of the best construction. As to the beds, those of the Parisian hospitals are much cleaner than the beds of the Hambourg Hospital, or even than most of the couches used by the lower classes in the north of Germany.

The Hambourg Hospital contains from 1400 to 1500 patients. M. Sandtmann is the senior physician, and under his care is placed *the whole* of the patients in this vast establishment; the surgeon is M. Fricke. Thus, 1500 patients are placed under the sole medical direction of *one* physician, and, were he to give only one minute to each patient, he would require a day containing twenty-five hours—one of twenty-four would be insufficient.

LUBECK.

Lubeck contains an asylum for the insane. On asking to see it I was told that it was in too poor a condition to be shown, but this only stimulated my curiosity. The establishment at Lubeck is a small square building, one story high; the ground-floor contains a single corridor, divided into cells, which are separated by naked planks; each cell receives light through a grated window, from one of the bars of which hangs, by a long chain, a species of spoon, or ladle, for the administration of fluids; each cell contains a wooden bench and a feather bed; the cells of the male patients are opposite those of the females, but both sexes assemble together in the same court-yard. When the insane patients are attacked by any severe disease they are treated in their cells, as there is no sick-ward prepared for their reception.

ST. PETERSBURG.

Four days after my departure from Lubeck I arrived at St. Petersburg. Nowhere do there exist more beautiful or richer establishments than the hospitals of the Russian capital; they are all recently built, and admirably adapted for their destination, except, perhaps, the Lunatic Asylum, which is a little inferior to the first-rate establishments for the insane in France.

The principal hospitals of St. Petersburg are those of the Medico-Chirurgical Academy, of Oboukhof, of Vassili-Ostrof, and the Imperial Hospital; these four are destined for the reception of persons affected with ordinary diseases; in addition to these are the Venereal Hospital, the Foundling Hospital, the Asylum for Orphans, and that for the Insane. The Medico-Chirurgical Academy was founded in 1718 for the purpose of affording instruction to the medical officers of the army and navy; the professors are all Germans by birth or extraction, with the exception of a single Russian; the hospital attached to the Academy contains three thousand patients.

The Hospital Oboukhof is the Hôtel-Dieu of St. Petersburg; it is composed of two distinct buildings, separated by an extensive garden; one range of buildings is of solid construction, and is destined for the winter habitation of the patients; the other is lightly built, has only one story, and is occupied by the patients during summer; the wards are of various sizes, some contain two beds, others not less than twenty; the bedsteads are all made of iron, and extremely well furnished; the floors of the wards are planked with wood, and varnished, so that they may be frequently washed without absorbing the humidity. Immediately on the reception of a patient into the hospital he is placed in a warm-bath, and then furnished with a complete clothing of excellent materials, a napkin, towel, and pocket-

handkerchief. Whenever a patient is afflicted with severe disease a small bell is placed on the table near his bed that he may command immediate attendance; the plates are of various sizes, and each bears a particular mark corresponding to full, half, quarter diet, &c; the plate is always filled, and in this manner the quantity of nourishment given to each patient is determined with the utmost nicety; the interior of the wards is warmed by means of enormous stoves, placed at convenient distances, so as to keep the temperature of the apartments nearly at 14° R.; the corridors are furnished with carpets, to prevent the sick from being disturbed by the noise of those who may take exercise in them. M. Mayer is the senior physician of the hospital; under him are eight paid and six unpaid physicians, together with a number of assistant-surgeons. The hospital contained, at the time of my visit, 454 patients, of whom 304 were males, the rest females; one of the assistant-physicians has the charge of one or more wards; his duty is to write out the history of the case (in Latin), which he attaches to the head of the bed,—to examine and prescribe for the patient; the assistant-surgeons, or, more properly speaking, dressers, are young men selected from the class of slaves, or from the Foundling Hospital; they receive a kind of half medical education under M. Mayer, administering the remedies, performing the minor offices about the patients, &c.; at the expiration of *twenty years'* service they become freemen. The mention of a few circumstances will suffice to demonstrate the extreme attention which is paid to the patients in the hospitals of St. Petersburg. In many of the wards there are glass cases, containing the various articles necessary for dressing wounds, sores, &c. Patients attacked with gangrene are immediately removed to distinct wards. The corridors and wards are constantly purified by burning perfumed substances. There is an abundant supply of hot and cold water in all parts of the establishment, and the water-closets, which are perfectly inodorous, are warmed during nine months in the year.

The Venereal Hospital exhibits the same degree of elegance and cleanliness as the other establishments of St. Petersburg. The senior physician is very partial to mercury, especially the corrosive sublimate, and places but little confidence in sudorifics, &c. The nourishment is wholesome and in abundance: bread, meat, and vegetables; wine is sometimes administered, but the ordinary beverage is a fluid called *quass*, a fermented liquor obtained from barley and barley-rice, rendered aromatic with some infusion of peppermint.

The Foundling Hospital is placed under the direction of Dr. Doepp, assisted by twenty ordinary physicians; it contains no less than *eighteen thousand* children of both sexes. No expense is spared on the care of its little inmates. In a country like France such an establishment would be a frightful evil, but in Russia it is the source of unlimited good. Children abandoned by their parents are received without any distinction or inquiry; any poor person, unable to support his child, may bring it to the hospital. The average number of admissions is five thousand during the year; eight hundred nurses are constantly employed, some in suckling, others in readiness for infants which may be brought in. These women usually remain five or six weeks in the hospital, and during their residence are taught habits of order and cleanliness. Each child is vaccinated before it is entrusted to them. In spite of all this care the mortality amongst the children is very great. During the space of ten years, from 1822 to 1831, of 39,114 children received, no less than 31,779 children died; the survivors are supported in the hospital until they are twenty years of age; they receive an excellent education, and are taught some trade; children who evince greater capabilities receive a higher degree of instruction, according to their talents. When discharged from the hospital the boys obtain employment in the various government establishments, while the girls are chiefly placed out as governesses in the families of the rich. The Emperor Nicholas frequently visits the Foundling Hospital, which enjoys his special protection, and omits no opportunity of contributing to the comfort of its inmates.

(To be continued.)

METHODS OF VACCINATION.

To the Editor of THE LANCET.

SIR:—At the last meeting of the Royal Medical and Chirurgical Society, holden this day, December 11, 1838, Dr. Merriman (in consequence of a report issued from the Small-pox Hospital, showing the astounding increase of patients affected this year, more than any other, with small-pox after vaccination) introduced the subject of small-pox, and called upon Dr. Gregory, who was present, for a statement of facts; but, without anticipating your report, I shall proceed at once to notice my query, thereby trusting to draw the attention of the profession to it, through the medium of your useful Journal, the President, Dr. Bright, suddenly ending the debate, to make room for the proper (or alloted?) business of the evening.

My question to Dr. G. was,—what were the greatest number of cicatrices he had observed on the arms of patients admitted with small-pox after vaccination? Dr. Gregory replied, that he had counted one having five cicatrices, but the prevailing number was either two or only one cicatrix.

Although Dr. G. did not mention that the cases frequently presented three cicatrices, I am willing to admit that they may, but must confess I have never witnessed a case where the cicatrices exceeded three; in this particular I am, or was, some months back, supported by the Messrs. Leese, of Baker-street, who have the sole management of the Vaccine Establishment in that district. Their opinion on this subject, at the present moment, would be truly acceptable, and where is there a better channel than the pages of THE LANCET?

I am anxious, then, to ascertain whether the failure of vaccination may not be rather attributable to the constitution of patients not being duly saturated with the virus introduced into the system, than to the degeneration of the matter used, and, therefore, would strongly recommend that every practitioner should obtain not fewer than four pustules of full size and maturation,—less than these not to rest contented with. It is my practice to make six punctures, three in each arm, except where I am fearful, from any peculiar circumstances in a family, ascertained from prior results, operating against it, I then content myself with making only three; but this is very rare.

The practice I advocate I was led to adopt from the result of secondary vaccination; for I observed, to my no small surprise, that the fewer the number of old cicatrices observable on the arms of patients who presented themselves for revaccination, the more sure I was to obtain a speedy and successful issue. I am, Sir, your obedient servant,

JAMES HERBERT HORNE.

8, Dover-street.

DISTRIBUTION OF LYMPH AT THE SMALL-POX HOSPITAL.

REPLY OF DR. GREGORY TO MR. CHATTO.

To the Editor of THE LANCET.

SIR:—Mr. Simpson having again taken the field, with Mr. Chatto for his ally, I revert to the subject, which, as I told you, was not exhausted in my first communication. I cannot but regret that Mr. Chatto declined favouring you with the whole of the correspondence that passed between us, including his own polite and conciliatory letter. Had he done so there would have been little occasion for my trespassing again upon your columns.

Mr. Chatto seems as unwilling as Mr. Simpson to acknowledge the essential differences between the constitution of the Small-pox Hospital and that of the National Vaccine Establishment. The latter is supported by the country, not for the purposes of charity, but for the benefit of metropolitan practitioners. One thousand eight hundred pounds per annum are devoted, almost exclusively, to their accommodation. The Small-pox Hospital, on the other hand, is supported by the voluntary contributions of the charitable for the purpose (*inter alia*) of affording the benefits of vaccination to the poor and working classes of the metropolis. For them our doors remain open from ten to one daily; and it is the duty of the medical officer to provide an adequate supply of lymph for all who may apply until one o'clock has passed. If a parent should arrive with her infant at that hour, from Hornsey or Camberwell, after a two hours' walk, and find that the whole stock of lymph had been disposed of to medical practitioners, she would have strong grounds of complaint, though, wanting the ready pen of a Simpson or a Chatto, she would probably not seek in the pages of THE LANCET either for redress or sympathy. To meet such a contingency it is often the disagreeable duty of Mr. Marson and myself (in the exercise of the discretionary power vested in us by the Governors of the Hospital) to keep medical men waiting, not for half an hour, but for an hour and a half, and, perhaps, ultimately to disappoint them. I am glad of the opportunity of stating this, because, as the winter advances, the necessity of such delays will be imposed upon us each day more and more. It frequently happens at this period of the year, that, for weeks together, the supply of lymph is inadequate even for the wants of the institution. It is easy for gentlemen to look round, and see what appears to them an adequate supply; but on them no responsibility rests. Regardless, therefore, of the frowns of Messrs. Simpson and Chatto, my colleague and myself will continue to believe, so long as we hold office, that our first and paramount duty is to provide for the wants of those for whom the charity is designed. Out of our abundance we shall always be happy to assist our professional brethren, but the power of deciding on the amount of such surplus must be left to the officer of the establishment; and, permit me to add, no one is competent to judge of the discretion with which that power is wielded, but he who has watched carefully the working of the machinery for many successive seasons.

The stations of the National Vaccine Establishment are numerous, and open, as a matter of right, to all professional applicants; nor do I clearly comprehend how, with such apparent opportunities around them to satisfy their utmost wishes, Messrs. Simpson and Chatto should evince so much anxiety for the reform of an establishment to which they neither contribute individually, nor as members of the general community. Is it that the zeal of such reformers might usefully be exercised on institutions with which they have a more natural connection?

Whatever be the opinion which these gentlemen entertain towards us, we have the satisfaction to think that public opinion is not against us. Already, during the present year, we have vaccinated upwards of 4700 persons (the largest number ever vaccinated at the Hospital in one year) and furnished lymph to nearly 1500 applicants. So long as these proofs of public favour are bestowed, my colleague and myself can well afford to be shown up occasionally in THE LANCET.

One word at parting (in perfect good humour) to Mr. Simpson. He says, in his last letter to you, that there exists in the world a class of people not easily put out of conceit of themselves. In this statement I cordially acquiesce, and could point out an apt illustration where, probably, he himself would never think of looking for it. But I can do more; I can remind him of another class of persons, occasionally met with in the world, and well typified in the fable of "The Old Man and his Ass,"—persons whom, do what you will, it is impossible to please. I am, Sir, your very obedient servant,

GEORGE GREGORY, M.D.

31, Weymouth-street, Dec. 17, 1838.

OBJECTIONS TO DR. WILKS'S MODE OF "REDUCING ARSENIOUS ACID."

To the Editor of THE LANCET.

SIR:—Your Journal of to-day contains a communication from a lecturer at a medical school, Dr. Wilks, which he calls, an "observation on the reduction of arsenious acid by the flame of hydrogen gas," not applying his language to what is the case, namely, that the arsenious acid is already decomposed, and the metallic base itself combined with the hydrogen, before he ignites the gas, and produces a flame.

The improvement of Dr. Wilks must be an improvement on some previous rude construction of his own; it certainly is no improvement on the elegant apparatus of Mr. Marsh, of which he will find a description in the second volume of the "Transactions of the Society of Arts." He will there find that the apparatus in question is not a bottle, but a tube, bent in the form of a syphon, the long limb being open, and the short one, in which the gas can be collected without admixture with atmospheric air, is terminated by a cork, in which is inserted a small jet with a stop-cock; that the gas, when lighted, is so far from being dispersed, that, by holding a piece of glass (not over the flame, as stated by Dr. Wilks, but in contact with it) a distinct metallic crust can be obtained from a single drop of Fowler's solution, which contains but the 120th part of a grain of arsenious acid, a quantity which is twenty times less than that he mentions; that, by holding a glass tube over the flame, arsenious acid may be collected in it; and, also, that, by holding the same tube obliquely, a metallic crust, arsenious acid, and the test of the garlic-like odour, may all be obtained together.

Not to mention the risk of explosion from the mixture of hydrogen with the air of the bottle and tube, and that the end of the glass tube would be apt to fly into pieces from the heat of the flame, the grand objection to Dr. Wilks's proposed improvement is, that when there is but a small quantity of arsenic in the fluid operated on, it will all be gone with the first portion of hydrogen, before the gas comes over sufficiently "briskly" to be inflamed.

To conclude: although a bottle with a bent glass tube may be better than a bottle with a straight glass tube, still the contrivance which Mr. Marsh says may, in an emergency, be had recourse to, viz., a phial and a pipe stopper, would be infinitely better than either. I have not the honour to be known to Mr. Marsh. I remain, Sir, your obedient servant,

CANDIDUS.

December 8, 1838.

LOSS OF BRAIN WITHOUT DERANGEMENT OF THE MIND.

To the Editor of THE LANCET.

SIR:—In consequence of my not having visited Liverpool for the last three or four weeks, I have neglected to supply myself with THE LANCET. I was, therefore, not aware of the communication which referred to my case of compound fracture of the os frontis contained in your Journal of the 3rd ult. I now beg to state, that I am not a disbeliever in the science of phrenology generally; but in witnessing facts, which I considered to be, in some degree, opposed to its principles (furnished by the occurrence of this accident), I felt called upon to give them notice and publicity. Phrenologically considered, this case was beyond my comprehension; I knew it to be interesting, and I wished for information. The fracture extended in a direct line across the frontal bone, commencing at that part of it which is indicated by the outer line of the organ of *time*, of one side, according to Spurzheim, sweeping away the *whole* of *eventuality*, and the two *localities*, the mischief bordering slightly on *time*, and the organs situated immediately both above and below, and penetrating into and carrying away *the substance of the brain* for *fully one inch in depth throughout the space of the wound*. Of the poor girl possessing her

mental faculties perfect, until within an hour of her death, all I can state is, that she constantly betrayed, both by speech and action, all those signs which are regarded as denoting *intelligence*. Two instances of this particularity were peculiarly obvious. Her attendant gave her an ear of wheat, and requested her to eat the grain; but after making the attempt, she said it was *too hard*, she could not chew it. The day before her decease, she also sung a verse of a hymn in as distinct and perfect a manner as she had ever before exhibited. She was also remarkably acute in the power of discrimination, nor, indeed, was she apparently deficient, except in great prostration of physical strength (a circumstance not to be wondered at), in any one sense or faculty, either of body or mind, that I could perceive. I am, Sir, your very obedient servant,

JOSEPH HARDING DRAWBRIDGE.
Surgeon, M.R.C.S.L.
New Ferry, Cheshire, Dec. 13, 1838.

REMUNERATION OF MEDICAL ASSISTANTS.

To the Editor of THE LANCET.

SIR:—Having noticed in your last week's Number of THE LANCET, some remarks upon the advertisement for an assistant, "Salary £8 per ann." which pleased me exceedingly, I beg to submit to your notice one which appeared in "The Times" of Thursday or Friday last, requiring "a middle-aged assistant," to dispense and visit, for a salary not to be more than £20 per annum. Address "7, Surrey-square, Old Kent-road." The advertisement states that there are 14 parishes, and other medical appointments, the duties of which must, of course, devolve upon the "middle-aged assistant" with £20 per annum. Being somewhat surprised that any medical man in the Old Kent-road should have so extensive a practice, I inquired at the above address, and found that the advertiser resides in a secluded part of Wiltshire, about eight or nine miles from Devizes, on the Salisbury-road; that the other "medical appointments" consist of about 12 medical clubs, added to which there is an extensive private practice.

Now, Sir, though it is pretty generally admitted, that medical men obtain but a very inadequate remuneration from the English] Bastile Directors for the services rendered to the in and out-paupers, still this admission is balanced by the feeling that the practitioners themselves, and none others, are to blame. Why then should "middle-aged assistants" suffer for the foolish tenders of their junior superiors, and such juniors, who, to prevent abler men from obtaining a fair recompense for "real professional services," enrol, in medical clubs, families fully capable of paying for sound advice, but, enticed by the cheapness of the annual subscription, are caught in the trap of the jockey apothecary?

Imagine Sir, a practice of £600 per annum, in an agricultural district, the parishes of which, "14 in number," must of necessity lie widely apart, and consequently "if actively engaged," the middle-aged assistant must perform the greater share of the labour. Are not £20 and seclusion from Society, inadequate to repay the poor assistants? I am, Sir, an

OBLIGED CORRESPONDENT.
London, Dec. 14th.

HYDROPHOBIA WITHOUT ABRASION OF THE SKIN.

To the Editor of THE LANCET.

SIR:—I beg the favour of you to insert the following observations (if worth a place in your valuable Journal) at your earliest convenience. Yours obediently,

THOMAS H. SILVESTER, M.D.,
Member of the College of Physicians.
Clapham Common, Dec. 14, 1838.

It may not be uninteresting to those who were engaged in the discussion at the London Medical Society, on the subject of hydrophabia, to learn, that the fact of the communication of the rabid poison through the unbroken skin, and, occasionally, by a medium more rarefied than saliva, was well known to the great masters of our art. Archæus will be found to express himself as follows:—

Αταρ και απο γλωσσης κυνος ισπνευσαντος μονον, ες την αναπνοην, ουτι μην ενδακοντος λυσσα ο ανθρωπος.

Which may be thus translated:—"And, again, a man becomes rabid from the tongue of a dog merely breathing on him, in expiration, without his being at all bitten."

Aurelianus also affirms that the inhalation of the vapour from the mouth of a dog in the rabid state has been found sufficient to produce the disease.

Galen held a somewhat different opinion, for although he confessed the truth of the generally received doctrine, that the poison readily passed through the sound, unabraded skin, he thought the presence of saliva almost necessary as a medium for the propagation of the disease.

*Ωστε το σιαλον αυτον μονον ανθρωπινω σωματι προσπιεσον εργαζεται λυτταν.**—Gal. lib. vi., cap. 5.

Fracastorius, and some others, from whom, probably, the moderns have derived their opinions, declared the impossibility of com-

* "So that the spittle alone, falling upon the human body, produces rabidness."

municating canine madness, excepting through a lacerated wound. This notion, however, rested solely on theoretical views in regard to the relative size of the particles of the poison and those of the blood, and is much less to be depended on than the deductions of ancient experience, and inimitable accuracy of observation.

DR. ELLIOTSON AND DR. ROGERS.

To the Editor of THE LANCET.

SIR:—An advertisement on the cover of your Number for November 3, introduces my name in a way which seems to demand a little explanation. The most satisfactory way of giving it will be to lay before your readers the following short correspondence. The first note is one which, in consequence of the above-mentioned advertisement, I addressed to Dr. Elliotson:—

"Sir:—In THE LANCET for last Saturday appears an advertisement, which contains the following passage:—'Dr. Elliotson writes thus to the publisher of his lectures:—"A gentleman, of the name of Rogers, dating from Kentish-town, has applied to me for permission to publish my lectures, which I have *refused*." We are acquainted with two other gentlemen who have received similar answers.' Will you allow me to ask whether the words of your letter are here transcribed correctly; or, whether they are what the advertiser considers to be your *meaning*, put into his *own* words. The letter which I had the honour of receiving from you hardly seems to bear out that statement. What I understood from the letter was, that while you would not *identify* yourself with *any* edition, neither would you *oppose* it. It is true you expressed an opinion that the lectures were not *worthy* of publication; but the rest of the profession think very differently on that subject. I am, Sir, &c.—November 8, 1838."

The following extracts from the Doctor's reply, are all that it is necessary to trouble you with:—"Dr. Elliotson presents his compliments to Dr. Rogers, and begs to say, that he has no recollection of having written to the effect mentioned. He could not have intended to mean that he had forbidden Dr. Rogers to publish them. He could have meant only, that he declined having anything to do with the publication.—Conduit-street, November 12."

Hoping you will grant me the short space in your columns that this explanation will occupy, I am, Sir, your obedient servant,

N. ROGERS, M.D.

3, Sussex-terrace, Kentish-town,
December 11, 1838.

UNIVERSITY COLLEGE HOSPITAL.

DISEASE OF THE WRIST-JOINT.—AMPUTATION.

J. E., aged 63, was admitted Sept. 11th, under the care of Mr. Liston; he is of temperate habits, and appears constitutionally healthy. Three months since he first noticed a soft swelling about the left wrist, which had previously become somewhat weakened. The swelling gradually enlarged, in spite of a succession of applications, consisting of poultices and blisters, iodine and mercurial ointment. Ulceration, unaccompanied with discharge, or bleeding, at the time. The wrist had previously been in perfect health, and he recollects no blow, or other cause for the appearance of the tumour.

There is at present a large irregular swelling involving, apparently, the heads of the radius and ulna, and the carpal bones, but not penetrating into the joint, since the motion of pronation and supination are fully, and those of flexion and extension partly, performed without grating, &c. The tumour is elastic to the feel, and in some places very soft, especially upon the thenal aspect, where, at one place, fluctuation is evident. On this surface also are two ulcerated openings, one on the ulnar, the other on the radial side; they seem to have been more extensive, marks of cicatrisation being evident around them. Their appearance now is nearly healthy, but their edges are raised, and they discharge a thin, curdy fluid. The integument is healthy in its colour at the back of the diseased part; this is the case also in the front, with the exception of a little reddening round the openings, and at the soft point where fluctuation is felt. The radial and ulnar arteries pulsate just under the integument and above the morbid growth; the veins on the back of the wrist are slightly enlarged as they pass over the tumour. Bowels open; tongue clean; no signs of fungoid disease in any other part. Mr. Liston pierced the integuments at the softest point with a lancet, and gave exit to about an ounce of pus mixed with blood. This proceeding, however, did not either materially diminish the bulk of the tumour or its elasticity to the touch.—Water dressings to the wound.

Sept. 17. There has been no further discharge from the wound made on the 12th. The part to be strapped with lead plaster and bandaged.

25. The same treatment has been pursued since the last report. The wrist is now diminishing in size; a considerable quantity of semi-purulent matter is discharged from the opening.

Oct. 6. Nothing remarkable since last report. To-day, however, there is much inflammation about the wrist, attended by pain and throbbing, with an erysipelatous blush on the surface; swelling very much

increased.—Leave off the strapping and bandages, and apply fomentations to the part.

7. Inflammation increased; fluctuation is plainly perceived. Mr. George made an opening on the dorsal surface between the thumb and index finger; an ounce of pus escaped, and the patient was somewhat relieved.—Poultice. A quarter of a grain of extract of aconite every third hour.

10. Erysipelatous blush nearly gone. The wrist is altogether better, and is much smaller.—Omit the medicine.

11. The swelling of the hand and wrist still less; the discharge, however, still continues copious and fœtid; the articular surfaces appear to be involved in the disease and separated from each other; the ulna possesses much more than natural motion.—A sixteenth of a grain of extract of belladonna was now given every third hour.

13. Discharge more copious, and the general health declining, Mr. Liston, therefore, determined on amputating the hand.

15. The hand and wrist were removed today in the usual manner, the flaps being formed from the thenal and anconial aspects of the forearm.

Nothing remarkable occurred during the further progress of the case; a slight erysipelatous blush took place round the wound a few days after the operation, but this was soon subdued, and the patient has continued to improve steadily up to the present time. He is now retained in the hospital for stricture of the urethra, which is under treatment.

DISEASE OF THE FOOT.—AMPUTATION. DEATH.

F. B., aged 19, was admitted Sept. 19, under the care of Mr. Liston. He is a linen-draper, scrofulous, and of an irritable temperament. A short time since he noticed a slight pain over the arch and on the sole of the right foot, which came on without any apparent cause; swelling supervened and the pain decreased. He merely applied a poultice to it previous to his admission into the hospital; there is now considerable swelling over the dorsum of the foot, and fluctuation is plainly felt. Mr. Liston opened the tumour in two places; much pus escaped.—Water dressings.

Sept 24. There is much grating in the joint formed by the first metatarsal and first cuneiform bones, and on introducing a probe through the opening on the dorsum of the foot the bones are found denuded and bare. As hectic symptoms have come on, Mr. Liston proposed to remove the great toe and the internal cuneiform bone; and on the 5th, the patient having consented, the operation was performed. The incision was made to form a lateral flap, which was dissected back as far as the scaphoid bone; the operator then removed the great toe and the first metatarsal bone; the joint between it and the cuneiform bone being loosened by disease, the latter bone was also removed: lint dipped in cold water was then applied to the wound. In the evening Mr. George tied four vessels, and brought the edges of the wound together by isinglass plaster. Water dressing applied to a small surface left uncovered anteriorly. He complained of considerable pain, and was ordered an anodyne.

A considerable oozing of serum took place all night, having commenced immediately after the dressing of the wound; his general health is rather bad. Pulse 136, weak; tongue moist, but furred; skin hot and dry.

It is unnecessary to give a detail of the progress of this case, which was unfavourable; the strength of the patient gradually gave way; gangrene seized the flap, and he died on the 19th of October, "being determined," as Mr. Liston observed, "not to live."

Necropsy.—No adhesion had taken place on the wound, the flap was sloughy, and a considerable slough had formed on the outside of the foot from pressure. There was also an infiltration of bloody pus into all the tissues and articulations of the tarsus, also into the ankle-joint. In some places the cartilage was eroded, but there was no apparent disease of the bones.

Chest.—There were considerable adhesions of the pleura; on breaking them up pus escaped from an abscess external to the right lung; this lung was gorged with frothy blood, and its texture soft and easily broken down. The left lung was gorged, but not so much, and its apex studded with tubercles.

Abdomen.—A collection of pus had formed between the spleen and the diaphragm within the cavity of the peritoneum, and was surrounded by an adventitious membrane; there was no extravasation of pus amongst the intestines.

DISEASE OF THE ANKLE-JOINT.—AMPUTATION. A SHE QUACK.

J. T., aged 25, was admitted March 27th, 1838, under the care of Mr. Liston. He is a horse-keeper. Last July twelvemonths, being under the influence of mercury, his feet were exposed to wet soon after which he felt a gnawing pain under the left heel. He became, in consequence, a patient in Guy's Hospital, where blisters were applied twice, and relieved the pain, and he was discharged. Eight or ten weeks afterwards the pain again supervened, but was removed by blistering, and bathing subsequently with salt and water. From this time it continued well till last June, when he became an out-patient of St. Thomas's for three months, and subsequently an in-patient for fourteen weeks, during which time blisters were applied; but as he did

not appear to be improving, he left the hospital. After this he employed salt and water to bathe the ankle, till three weeks since, when he became an out-patient under Mr. Liston. The ankle was then encased with ceratum saponis and straps of lead plaster, so as to prevent motion in the joint, under which treatment it appeared to improve.

Present Symptoms. — He complains of great weakness about the ankle, which is very little larger than the other. Putting the foot to the ground is productive of considerable pain. There is a quantity of matter collected about the external malleolus.

Treatment. — Mr. Liston evacuated the matter; water dressing to the wound, and the limb to be kept at rest on an inclined plane.

March 30. Wound granulating. A swelling and fluctuation over the internal malleolus.

April 4. Mr. Liston made an incision over the internal malleolus, from which a peculiar white flaky matter was discharged.

9. Purulent matter is now discharged from the opening last made. The ankle, this morning, is very much swollen, tense, and painful; poultice to be applied.

10. A great discharge of purulent matter, and swelling, with pain, still continues.

16. The swelling and pain have in some degree subsided under the application of the poultices, which are now exchanged for water dressing. Wound over external malleolus nearly healed.

24. Discharge less; pain and swelling also much diminished, but still great pain upon any attempt to flex or extend it.

May 13. No alteration. There is evident disease of bones of ankle.

June 6. The ankle was put up in leather splints, so as to steady the joint, and see what benefit might be experienced from keeping the part perfectly at rest.

12. Potassa fusa was freely applied to the opening.

July 2. It being found impossible to effect a cure, he was recommended to have the portions of carious bones removed; but preferred going to the sea-side, to try what bathing might do for him, previous to undergoing the operation, and consequently left the hospital.

He was re-admitted October 13th, 1838. Instead of going to some watering place, as he intended when he left the hospital, he put himself under the care of a she-quack, who applied some ointment and lotion to the part. During the time he remained under her treatment he had a fall, which injured his ankle, and caused it to swell greatly. He then went into St. Thomas's Hospital, where he remained eleven weeks, during which time linseed-meal poultices were kept constantly applied, but without affording any other relief than that of abating the swelling and pain. The primary disease still remained uncured. It was then proposed to him to have his leg removed, and he left the hospital, determined, if such a step were necessary, to have the operation performed by Mr. Liston. For this purpose he has again applied at this hospital. There are now two fistulous openings on the inside, and one on the outside of the ankle, which discharge continually; when a probe is passed into these, the bones are found rough and bare.

October 15. The patient was placed on the operating table, and Mr. Liston removed the diseased leg in the usual manner of the flap operation, about two inches below the knee. The popliteal artery alone required to be ligatured. A piece of lint, dipped in cold water, was then placed on the wound, and the patient carried back to bed.

Vespere. Three small muscular branches were secured, and, in two hours afterwards, the surface of the wound being glazed, the flap was put up, and secured by three points of suture, and strips of isinglass-plaster. Ordered an anodyne.

16. Passed a good night, and feels pretty comfortable; pulse 110; tongue slightly furred; skin and countenance natural. Complaining a good deal of pain towards evening, he was ordered another anodyne.

17, Stump looking well; some oozing of serum; slept well last night, but not so well this morning in general health. Has some difficulty in breathing, with pain in the chest, and cough; pulse 120; tongue furred; bowels not opened. To take an ounce of castor oil. This not relieving him, in two hours afterwards he took a dose of house-medicine, which produced a copious evacuation of the bowels. A quarter of a grain of tartarised antimony every third hour.

18. Better to-day; breathes easily, and the pain in the chest is gone; cough still continues. Decrease the dose of the medicine to one half. The sutures were removed, and some strips of plaster changed.

19. Going on well; pulse 112; tongue clean; bowels open; skin natural; countenance cheerful; cough continues, but no difficulty of breathing; no pain in the chest.

He went on steadily improving till November 9, when he was allowed to go home for a week.

IMPERFECT AND UNNECESSARY INQUEST.

Mr. George Lucas, surgeon, of Ilkeston, in Derbyshire, has communicated to us another instance of the neglect of coroners to call medical evidence at inquests. Mr. Lucas says,—"I was lately requested to

visit a person who was reported to have gone out to work without complaining of any unusual sensation, and apparently in good spirits, who suddenly fell down and speedily died. On my arrival, vitality having fled, I asked the relatives to permit an examination of the body; they readily gave consent, but recollecting that an inquest would be held, I abandoned the intention for the present. I afterwards heard that the inquest was held, that no post mortem examination was thought necessary, nay, was even condemned, at the inquest, and that a verdict of 'Died by the visitation of God,' was returned."—This inquest is clearly one of those which fall under the class UNNECESSARY. To institute such sham investigations into the cause of death is perfectly useless. A protest should be entered against payment of any fee to the Coroner for holding what he has himself pronounced to be, by the rejection of a post-mortem examination, *an unnecessary inquest*. The commonplace verdict of "Died by the visitation of God," is one of the most dangerous inventions of the time. It is the constant subterfuge of ignorant coroners and ill-guided juries, and serves at once to dispense with medical evidence, and defeat the wise purpose of the coroner's court.

MR. GRAINGER.

To the Editor of THE LANCET.

SIR:—As your remarks on certain proceedings that have been perpetrated at Webb-street, materially affect the character of our respected teacher, Mr. Grainger, will you permit me to state, that I, in company with Messrs. Bradden and Taylor, heard Mr. GRAINGER declare his total dissent from that resolution which approves of the existing Medical Corporations; and when surprise was manifested that he did not so express himself in the meeting, he said, that having been requested to preside on that occasion by the students, he was unwilling to bias their opinion by the expression of his own; at the same time, he felt not only grieved but surprised at their decision. Deservedly does that decision call for your heaviest censure; but I most sincerely hope, that after this explanation you will still consider our illustrious teacher as "one of the faithful." Yours, &c.

A. C. LEE.

Three Crown-square, Borough,
Dec. 17, 1838.

[We publish the above note with feelings of sincere satisfaction. We confess that the merits and public spirit which were displayed by the late undaunted FOUNDER of the Webb-street School, have established, in our minds, a strong prejudice in favour of the name of GRAINGER.—ED. L.]

RECENT MEETINGS AT THE WEBB-STREET SCHOOL.

LETTER FROM MR. GRAINGER.

To the Editor of THE LANCET.

SIR:—Although I have no wish to obtrude my opinions on the notice of your readers, yet as the observations published in THE LANCET of Dec. 8th and 15th, in reference to two meetings lately held by the students of this School, are calculated to create a false impression of my sentiments on a question of great public interest, I must request you will do me the justice to give insertion to this letter in the next Number of your Journal.

It is not correct that I was in any manner instrumental in calling the second meeting, held on Nov. 28th, or in preparing the resolutions which were brought forward on that occasion, further than suggesting a verbal alteration in the second; nor is it correct that the movers and seconders of those resolutions were my private friends;—those gentlemen were known to me simply as pupils, and I in no degree influenced their opinions or controlled their actions. It is incorrect that I prevented, either directly or indirectly, any individual from expressing his opinions, whether those opinions were in favour or in condemnation of the propositions submitted to the meeting of which I was the chairman. It is not correct that I myself proposed and seconded the above resolutions, or that I took any other share in that part of the proceedings, beyond submitting the propositions to the decision of the meeting.

Having corrected the misstatements to which I conclude you have unintentionally given publicity, I feel it incumbent on me to notice the observations you have offered concerning my conduct on the above occasion. I have yet to learn that the chairman of a public meeting is responsible for the opinions expressed by the individuals who take part in its proceedings, or for the decision at which the majority ultimately arrives. If it should have been supposed by you or others that, from my position in this School, the present instance constitutes an exception to a general principle which none can be so absurd as to contest, I must deny *in toto* the justice of such a conclusion. At all the public meetings which have been held within these walls, I have scrupulously avoided whatever might have even the semblance of checking the freest expression of opinion; and deeply should I regret if I were myself capable of attempting, or if the members of this School were contented to submit to, such an unwarrantable interference. In thus disclaiming the responsibility you have endeavoured to attach to me, I do not shrink from

the avowal of what are my real sentiments touching the question of medical reform. I have on no fitting occasion, either public or private, hesitated to express my conviction that the existing medical corporations are vicious in their constitution, vexatious, and often unjust in their operation, and altogether incompatible with the enlightened spirit of the present age. These are the opinions I have unflinchingly, and without deviation, maintained throughout my professional career,—they have on no occasion been suppressed or concealed, and they are at the present moment unchanged. Entertaining, however, these ideas, I, for one, will never withhold from those bodies any merit to which they may be justly entitled; and when the state of the profession, as regards education, previous to the year 1815, is contrasted with its existing condition, I do conceive that those who have actively contributed to effect a change fraught with such inestimable advantages to the community, have some claim to the public gratitude.

But, Sir, the two meetings to which you have called the attention of your readers, had principally reference, not to the merits or demerits of the old corporations, but to the proceedings of the University of London. My participation in the transactions of those meetings was to this extent:—At the conclusion of both, and especially of the second, after the resolutions had been discussed and carried, I certainly stated my satisfaction that the intention of petitioning the Legislature had been abandoned; and I did so for two reasons,—First, because it would have been premature, in so far as no curriculum had emanated from the Council of the University of London; and, secondly, and especially, because I should have most deeply regretted any attempt to impede that body in the effort it was making to advance the utility and dignity of the profession, by securing an uniform and scientific education to the general practitioner, and by acknowledging no other title to honourable distinction than the possession of superior attainments.

The opinions expressed in this letter will, it is probable, incur the displeasure of many whose friendship I esteem, and whose private worth I venerate; but, however painful this may be to my feelings, I should, indeed, despise myself, if I allowed any views of private interest, or personal predilection, to interfere with the discharge of a public duty.

I cannot conclude without expressing the deep gratification I experience, in common with many others, who have for years lamented the evils resulting from the existing system, in witnessing a recognition of those great principles of equity, and of those enlightened views of education, which are embodied in the Report of the Committee of the University of London.

I have the honour to be, Sir, your obedient servant,

R. D. GRAINGER.

Theatre of Anatomy and Medicine,
Webb-street, Southwark,
Dec. 19, 1838.

*** The above letter reached us only a few hours before our Journal of this week was ready for press, and we most readily comply with the request of Mr. Grainger for its immediate insertion.

BOOKS RECEIVED.

Anatomical Plates, illustrating the Structure of the Different Parts of the Human Body. By Jones Quain and William J. E. Wilson. Division III., Nerves, XV. and XVI. Taylor and Walton, London, 1838.—[The reputation which these excellent plates have already obtained will not be diminished by the appearance of the fasciculi devoted to the nervous system. They are at once faithful and elegant representations of Nature.]

The Pathological Characters of the Urine. By Edwin Adolphus, M.D. Lee, London, 1838. 8vo, pp. 91.—[A very useful resumé of the state of our knowledge on this interesting subject.]

The Principles and Practice of Ophthalmic Surgery. Edited by A. C. Lee. Butler, London, 1839. 8vo, pp. 242.

An Account of some New Instruments for tying Polypi. By William Beaumont, Surgeon. Churchill, London, 1838. 4to, pp. 35.

The Medical Student's Guide to the Hospitals of Paris. By John Wiblin. Renshaw, London, 1839. Small 8vo, pp. 90.—[Contains some useful information, but there are several omissions, and the author evinces a most illiberal spirit towards the French students of medicine.]

TO CORRESPONDENTS.

S. P. Why is the *name* of "Mr. J., of Birmingham," concealed? We believe Mr. P. to be a respectable gentleman; we doubt not the honesty of his conviction; but he should remember that thousands of acute and equally respectable persons had been completely deluded by the humbug which he endeavours to prop. Let Mr. J. of Birmingham, try his hand on some persons besides *young ladies*.

If we insert the letter of *Medicus*, he must first send his name and address to be attached to the communication.

Communications have been received from *Messrs. Brown, Curtis, Attenburrow, Goodman, Observer, A Constant Reader, W. J. M.*, and *Medicus*.

The "Leicestershire Telegraph" has been received.